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TRANSACTIONS

OF

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MINING & METALLURGY,

LONDON.

1893 - 94.

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TRANSACTIONS

OF

THE INSTITUTION OF

MINING & METALLURGY,

LONDON.

THIRD SESSION,

1893-94.

THIRD SESSION.

ADJOURNED SECOND ANNUAL GENERAL MEETING & FIRST ORDINARY MEETING.

THE ADJOURNED SECOND ANNUAL GENERAL MEETING AND FIRST ORDINARY MEETING of the Third Session was held on Wednesday evening, November 22nd 1893, in the Lecture Theatre of the Geological Museum, Jermyn Street, S.W. Mr. J. H. COLLINS (Vice-President) occupied the chair.

The VICE-PRESIDENT, in opening the proceedings, said: Gentlemen—Our first business is to hold the Adjourned Second Annual General Meeting, and that will take us but a very short time. The proceedings will include the reception of the report of the Council, containing a statement with reference to the accounts for the financial year ending February 28th 1893.

The VICE-PRESIDENT then read the following Report of the Council:—

To the Members, Associates, and Students of the Institution.

Gentlemen,—Your Council have pleasure in submitting to you their Report for the year ending February 28th 1893, and, in doing so, regret that the severe and prolonged illness of their Hon. Secretary has prevented it being presented at the proper time.

They would briefly remind Members that the establishment of the Institution dates from the 13th January 1892, when a meeting of Mining & Metallurgical Engineers was held at Winchester House,

London, E.C., for the purpose of considering the expediency of forming such an Institution. The First General Meeting of the Institution was held, by kind permission of the Lords of the Committee of Council on Education, on May 18th 1892, in the Lecture Theatre of the Museum of Practical Geology, Jermyn Street, London, S.W. There was a large attendance of Members, Associates, and Students, and of gentlemen connected with the interests embraced within the scope of the Institution; and Mr. George Seymour, A.R.S.M., M.Inst.C.E., delivered his Presidential Address.

Since this time, 10 meetings have been held at the Museum of Practical Geology, at which the following Papers have been read and discussed:—"Mining in the Malay Peninsula: A Field for Mechanical Appliances, Hydraulicing, and Electric Transmission of Power," by Mr. H. M. Becher (Member). "Auriferous Ironsands, and a New Method of Profitably Extracting Gold therefrom," by Lieut. W. B. Basset (Associate). "The Industry of Mining," by Mr. A. G. Charleton (Member). "Gold Amalgamation," by Mr. C. G. Warnford Lock (Member). "The Decomposition of Auric Chloride obtained in the Chlorination of Gold-bearing Materials," by Mr. Claude Vautin (Member). "The Mining District of Oporto," by Mr. Frank Merricks (Associate). "Gold Mining in India," by Mr. A.

last Report was issued 120 gentlemen have been admitted to the privileges of the Institution.

GEORGE SEYMOUR, *President.*
GEO. A. FERGUSON, *Hon. Secretary.*

The VICE-PRESIDENT then said: With regard to the accounts and the publications, which are long overdue, the Council can only promise you that they will be out now as soon as possible. Circumstances which have been a good deal beyond control have occasioned much delay, but things are now in train for their proper issue, and it will not be very long before you will get all that you ought to have had in that way.

The report of the Council was adopted unanimously.

THE FIRST ORDINARY MEETING of the Third Session was then commenced. The SECRETARY (Mr. Ernest H. Peers) read the minutes of the last meeting, which were duly confirmed.

Professor A. K. HUNTINGTON (Vice-President) then took the chair while Mr. J. H. Collins read the following paper :—

ON THE ECONOMIC TREATMENT OF LOW-GRADE COPPER ORES.

By J. H. Collins (*Vice-President*).

SUMMARY.

Sec. I. Introductory :—

Definition of Copper Ores.
Variety of Copper Ores.
Low-grade Ores.
Chemical Classification :—
1. Sulphides.
2. Oxidised Ores and Haloids.
3. Native Copper.
General Remarks.

Sec. II. Mechanical Concentration :—

Classification.
Crushing and Wet-Concentration.
General Principles of Mechanical Concentration.

[redacted]

SEC. V. FURNACE-CALCINATION METHODS :—

General Remarks.
Kiln-Roasting.
Furnace-Calcination.
Bankart's Process.
The Linz Process.
The Longmaid Process.
Peña del Hierro Process.
The Hunt and Douglas Processes.

SEC. VI. COPPER-PRECIPITATION :—

Copper-Precipitation with Iron.
Consumption of Iron.
Character of the Precipitate.
Quantities of Water Used.
General Conclusions on Precipitation with Iron.
Electro-Deposition.
Precipitation as Carbonate and Hydrate.
Precipitation as Sulphide.
Precipitation as Chloride.

SEC. VII. RAW-SMELTING METHODS :—

General Conditions.
Calcination.
Kernel-Roasting.
Roasting of Fines in the Reverberatory Furnace.
Smelting to Matte.
Smelting of Fines in a Blast-Furnace.
Smelting to Black Copper.

SEC. VIII. CONCLUDING REMARKS :—

Scale of Working.
Choice of a Process.
Samples, Assays, and Accounts.
The Utilisation of Bye-Products.
General Conclusions.

Section I.—Introductory.

Definition of Copper-Ores.—In a general way this term may be held to include all minerals which contain copper in not too minute proportions, and to a greater value than any other substance present, although they may not exist in workable quantities. From the point of view of the practical miner the term is somewhat more limited in its application, for it implies the idea of workable, or at least considerable, quantity.*

Variety of Copper Ores.—A reference to any fairly complete work on mineralogy will show that there are considerably more than one hundred distinct mineral species which fall fairly within the terms of the foregoing definition, besides nearly as many more which are excluded by it though they contain normally from one-half up to 5 per cent. of copper. This great variety of copper ores contrasts strongly with the practical unity of tin ore, which can hardly be said to exist except as the oxide cassiterite, for the supposed sulphide known as stannite, or tin pyrites, has usually been treated as a copper ore, the tin being lost in the process of smelting. Even in stannite the tin appears to exist mainly, if not exclusively, as peroxide; however, this mineral has seldom occurred in such quantities as to be of much commercial or economic importance.†

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the most important sources of copper in the present day are certain "low-grade" ores, containing in general from less than 1 up to 3 per cent. of copper. Such are the vast masses of cupreous iron pyrites of the Sierra Morena and elsewhere, the amygdaloid and conglomerate beds of the Lake Superior region, with their disseminated particles of native copper, the bituminous schists of Mansfield impregnated throughout with sulphide ores, and many other disseminations and impregnations to be hereafter referred to. These are the ores with which the present paper is particularly concerned, though the tailings from richer ores may also come within its scope. Some typical examples are given in the following table:—

TABLE I.

EXAMPLES OF LOW-GRADE COPPER ORES.

A.—Ores Wrought for their Copper Alone.

	Name of Mine.	Copper Per Cent.	Remarks.
1891	Atlantic Mine, Lake Superior	0·65	Native copper in amygdaloid.
1891	Osceola Mine, Lake Superior	1·40	Native copper in conglomerate.
1882	Rio Tinto, Spain (calcination mineral)	2·20	Cupreous pyrites.
1882	Maidenpec (Brankovitz), Servia	2·00	Siliceous sulphides.
1882	Maidenpec (Staritza), Servia	3·00	Argillaceous and oxidized.
1875	Alderley Edge, Cheshire	1·40	Oxidized copper in sandstone.

B.—Ores Wrought for Copper and Bye-Products.

1878	Rio Tinto (exportation mineral)	3·50	Cupreous pyrites.
1879	Mansfeld, Germany	2·60	Bituminous schist.
1876	New Consols, Cornwall	1·50	Pyritous, siliceous, arsenical, &c.
1890	Devon Consols, Devon	2·00	Pyritous, siliceous, and arsenical.

In addition to the foregoing there is a vast number of ores and metalliferous spars, which, containing only small pro-

portions of copper, are either of greater importance for some other component (though they may yield copper as a bye-product when metallurgically treated), or else are of too rare occurrence to be of any importance as a source of this metal.

Chemical Classification of Copper Ores.—Ores of copper as here defined comprise almost every variety of form, colour, and lustre; the differences of density are very great, and the physical constitution ranges from very brittle, or even pulverulent, to an extreme degree of toughness. There is also great variety of chemical constitution, the copper occurring not only in the native state and remarkably pure, but variously combined with other elements, as sulphides, arsenides and antimonides, oxides, sulphates, carbonates, arseniates, phosphates and silicates, chlorides, bromides, and iodides; these, too, occurring both simple and compound, some anhydrous, others hydrated, some very soluble, others highly insoluble—including, in fact, in some form of combination, a great majority of the known elements.

In considering the nature and treatment of these different ores it will be found convenient to group them in the first instance as sulphides, oxidized ores and haloids, and native copper.

(1.) The Sulphide Class includes all unoxidized

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well as the silicates and haloids are mostly hydrous, though occasionally anhydrous.

(3.) NATIVE COPPER occurs in many forms, as grains, scales, plates, and lumps. It is occasionally associated, or even alloyed, with other metals, but is in general extremely pure.

SECTION II.—MECHANICAL CONCENTRATION.

Copper ores, of whatever chemical composition they may be, are seldom sufficiently rich as raised to be at once sold, so that some form of concentration at the mine is nearly always necessary, either mechanical, by "wet" chemical methods, by smelting, or by some combination of these. And, of course, this is absolutely essential with low-grade copper ores of whatever class, unless they contain components of value sufficient to pay all transit charges, apart from the value of the copper itself.

Classification.—For really low-grade ores, the simple and effectual method of hand selection is not generally applicable, and even when it can be used with advantage, as at Vigsnaes in Norway, there is nothing special in its application, so that it may be here passed over; in fact, the primary classification is rarely more than a rough screening or sizing, and even this is often omitted.

In general it is only ores of the first and third classes that are suited for the ordinary methods of mechanical concentration with the aid of water, while dry mechanical processes, though frequently attempted, have not yet proved commercially successful in any place known to the writer. Ores of the second class, unless of exceptional density and compactness, are found to suffer such heavy losses when finely crushed for wet concentration, that the method is only employed as a makeshift. Moreover, some ores like Cyanosite and Atacamite are actually soluble in water or in slightly acid solutions, while others like Chrysocolla hardly differ in specific gravity from ordinary veinstones.

Crushing and Wet-concentration. — The methods of mechanical concentration in ordinary use for copper ores scarcely differ from those employed for other metallic ores,

so that it is not necessary to do more than briefly refer to them here. For ores of the first class, and particularly for Chalcopyrite, also for those of the second class when mechanical concentration can be employed at all, stone breakers, crushing rolls, and jigs, are preferred in all cases, while stamping and buddling are only employed in the last resort, because of the serious losses which occur in the slimes thus produced.

A good example of mechanical concentration of a pyritous ore has long been afforded by the works at Vigsnaes mine, on the West Coast of Norway, where large bodies of cupriferous pyrites occurring in slate rock, and much resembling the Spanish ores are extensively worked. The ore as raised from the mine is hand-picked by boys into three sorts: 1st, clean pyrites in lumps and containing from two to two and a-half per cent. of copper, which is exported; 2nd, pieces of pyrites mixed with quartz or slaty rock; and 3rd, waste rock, which is comparatively small in amount. Class 2, together with the mine-smalls, is crushed by stone-breakers and rolls, and carefully sized by revolving trommels, each size being jigged in the ordinary way. The slimes are also dressed on Linkenbach revolving tables, it being found that the concentrates so obtained from the finest slime are considerably richer in copper than those from the coarser sizes. A similar process is used in most of the other pyrites mines of Norway, but it is more noticeable at

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powerful stamps and some of the most complete milling arrangements in the world are in extensive operation upon low-grade ores yielding exclusively native copper. For there are single stamp-heads of the steam-hammer type which crush over 200 tons of ore in each 24 hours; while some of the mines which have the advantage of an abundant water supply, without the necessity for pumping, are able to raise and treat ores of extremely low grade. Thus, at the Atlantic Mine, the cupreous amygdaloid only yields about 0·65 per cent. of copper, which, in the year 1891, was raised and treated at an average cost of 5s. 11d. per ton. This remarkable result was reached by a well-arranged system of stamping, followed by the use of V separators, jigs, and slime tables, and dealing with nearly 1,000 tons per day. The details of cost were as follows:—

	s.	d.
Mining costs, including hoisting, per ton	2	11
Transportation to mill, stamping, and dressing ...	1	2
Working and administrative expenses	1	1
Smelting to bars and freight to New York	0	9
	5	11
Profit	0	6
Value of Ore...	6	5

It is obvious that such results as these can be arrived at only under extremely favourable conditions, among which may be mentioned the great and uniform thickness of the ore-bodies, their unusual softness, the fact that very coarse stamping suffices to liberate most of the copper from the veinstone, and the cheap and abundant water supply, allowing of the use of nearly 40 tons of water in the treatment of each ton of ore.*

* At West Basset Tin Mine in Cornwall, about the year 1877, 37 tons of water were used in the treatment of each ton of tin-stuff for stamping and dressing. See Hocking, *Proc. Min. Inst. Corn.* II., p. 23, 1877.

The ore is crushed so as to pass through holes of about 3-16ths of an inch diameter, or slots of 5-16ths by 2-16ths, and it passes direct to the hydraulic separators, which are very extensively used at the Atlantic and other mines in the Lake Superior District. These consist of V shaped wooden troughs about 16 feet long, and constructed with an inner and an outer compartment. The inner trough is about 1 foot wide at the top, and 1 foot deep. The bottom is about 1 inch wide, and has four longitudinal slots cut in it, each about 1 inch wide, and from 4 to 9 inches long; the longest being at the head, where the stream of sand from the stamps enters. The slots open into the outer trough, which is of the same form as the inner, but separated from it by an inner space of about 3 inches. There are four openings in the bottom of the outer trough, each corresponding with one of the slots of the inner trough, and fitted with a discharge pipe, which leads direct to a jig. The outer trough is kept nearly full of water by a separate supply pipe, the surface being maintained at a little higher level than the stream of sands flowing along the inner trough. There are, therefore, upward currents through the slots which only permit the passage of those particles which are heavy enough to overcome their resistance; the heaviest and coarsest pieces falling through the first slot, the next falling through the second, and so on; while the slimes pass on to the settling pits. Thus a very complete

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stages of the concentration process without the necessity for handling—always a very costly affair where wages are high and boy labour is not to be had. Where the much harder conglomerate is worked the costs are, of course, considerably higher, yet the total working expenses rarely exceed 9s. per ton in the more-extensive and best-managed mines, such as the Osceola.

Of course, there is a certain loss of copper in this as in all processes. It consists partly of oxide of copper but principally of minute particles of metallic copper (float copper) which are carried away by the water, and partly, too, of minute particles enclosed in fragments of veinstone which have not been sufficiently reduced to liberate the copper. But the proportionate loss is certainly very small.

General Principles of Mechanical Concentration.—I am not aware of anything which calls for special remark in the mechanical concentration of copper ores at other mines, and I need hardly insist on the fact that the general principles are the same whatever the kind of ore. The ores must be crushed to the degree necessary to free the ore-particles from the veinstone but not to a greater degree, the greatest advantage possible must be taken of differences of specific gravity as well as of the prevailing differences of form subsisting among the particles, the works must be so arranged that the sequence of operations may be aided rather than impeded by the force of gravity, and the machinery employed must be as far as possible automatic in its action.

Section III.—Chemical Methods without Calcination.

Introductory Remarks.—The so-called chemical methods are very numerous, but they all involve the two processes of solution and precipitation. In dealing with this part of the subject I shall in the present paper confine my remarks almost entirely to those which have been, or can be, successfully employed by men possessed of practical experience and skill, but without systematic chemical training.

(1.) *Air Oxidation Process.*—This is the simplest of all processes, but it is scarcely applicable, except to ores of the (1st) class and not to all, even of them.* The raw pyritous ores are exposed, preferably in a moist state, to the oxidising action of the air. This process which goes on very rapidly with some varieties of cupreous iron pyrites, and in particular those of the Sierra Morena, is of course one of the cheapest, and doubtless, too, the oldest process of all, except the smelting of the richest picked ores in small blast furnaces. It may have been suggested by observation of what goes on in the old workings of pyritous copper mines or of the waters flowing from such, which are usually charged, and sometimes very highly charged, with soluble salts of copper and iron; or even from the observation of natural copper liquors flowing from deposits which have been laid bare by denudation. Or, it may have been first noticed in connexion with the waste heaps of such mines. Thus at Rio Tinto and other mines in the neighbourhood of the Sierra Morena, very considerable quantities of copper precipitate have long been obtained from the natural waters issuing from the adit levels or percolating through the waste heaps after rain.

Copper was precipitated from the waters of the Chacewater Mine in Cornwall, by Mr. Coster about 170

[redacted]

thus gone to waste in Falmouth Harbour since the opening of the Gwennap Mines a century and a-half before.*

By the year 1864 a mile and a-quarter of the valley was occupied by precipitation works, and the annual product of precipitate was about 90 tons, which varied from 6¼ up to over 60 per cent., with an average produce of about 40 per cent.†

These are the poorest natural copper liquors I have known to be worked. The richest were the so-called tunnel liquors at Rio Tinto, which, in the year 1883, from June to September, gave an outflow of 960 cubic metres per day—or, say, nearly 150 gallons per minute—containing nearly 50 grains to the gallon, or over 500 times as rich as the Cornish adit water.

Two other streams of copper liquor flowed from the Rio Tinto Mines, the average total outflow being, at the time named, about 480 gallons per minute, and the average copper contents nearly 26 grains per gallon, so yielding considerably over a ton of fine copper each day.

In the following table I have compared the yield of fine copper from certain natural mine waters at the periods named:—

TABLE II.

	Gallons per minute.	Grains of copper per gall.
Great Adit, Cornwall (1854-68)	9,000 ...	·08
Pary's Mountain, Anglesea (1862-66) ...	130 ...	1·30
Cronebane, Wicklow (1791)...	— ...	3·00
Wheal Josiah, Devon (1866)	— ...	4·80
Connoree, Wicklow (1838-9)	80 ...	6·00
Wheal Maria, Devon (1866)	— ...	7·60

* The calculation seems too low, it must have been more than twice that quantity.

† The waters still contain traces of copper now that the copper mines have been stopped for over 20 years, but the quantity is so small that the precipitation works have, I believe, at last been entirely given up.

	Gallons per minute.	Grains of copper per gall.
*Rio Tinto (1) Tunnel (1883)	150 ...	50·00
Rio Tinto (2), San Dionisio (1883) ...	90 ...	34·60
Rio Tinto (3), North Lode (1883) ...	234 ...	7·60
Rio Tinto, average of 1, 2, & 3 (1883) ...	480 ...	25·80

In all these, as in nearly all other cases, iron is the material used for precipitating the copper. The methods employed, and the results obtained, will be dealt with hereafter.

Naturally oxidised heaps of cupriferous refuse are often treated with advantage by causing water to percolate through them at intervals, alternating with periods of repose. The liquors so obtained are usually very much richer than those naturally issuing from adits or from old waste heaps, and they may be still further enriched by using them for washing heaps of crushed ore or natural raw smalls.

(2) *The "air-salt" process* is precisely the same, except that about 1 per cent. of common salt is added to the heaps of fines, with the objects (*a*) of hastening the oxidation, or of, at any rate, rendering the copper more soluble; or (*b*) of converting *small* traces of silver into chloride, which subsequently passes into solution with the copper, when the heap is washed by a flowing stream or lixiviated in tanks. Oxide of manganese has, in some cases, been added to the

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The best results in air-oxidation are realised when stores of burnt and partially washed ore from old heaps are available for mixing as hereafter described.

(3.) *Acid Solution Methods.*—The methods (1 and 2) are really effective because acid liquors are produced by the gradual oxidation of the sulphur. Direct acid methods are scarcely applicable except to ores of the second-class (oxides, carbonates, and haloids, but not silicates), and only then if waste acids are to be obtained cheaply, and where the veinstone is not acted upon by the solvent. At Alderley Edge, in Cheshire, hydrochloric acid was formerly used for that purpose. The ore at these mines consisted of sandstone rock, containing disseminated particles of carbonates and oxides of copper with little admixture of sulphides. This rock after crushing was treated with waste dilute hydrochloric acid from the St. Helen's Chemical Works, then a waste product. By this means a strong solution of cupric chloride was ultimately obtained, which was afterwards precipitated at a profit. A similar process was formerly in use on the same kind of cupriferous sandstone at Waldeck, in Germany, and doubtless in many other places, but with the present low price of copper it is hardly likely that such processes can be made to pay.

It is obvious that such a method would not be practically available in the case of sulphide ores, nor of ore particles embedded in a calcareous veinstone, even if already oxidised.

When the copper exists as metallic particles, the cheaper acids such as hydrochloric and sulphuric have very little action upon it. In such cases the use of the far more expensive nitric acid has often been suggested, but it has never been applied on the large scale, for obvious reasons, and, in fact, direct mechanical methods of concentration are far more effective in all such cases, as well as far cheaper than any chemical method yet proposed.

Section IV.—Open-Air Methods Involving Calcination.

Calcination in Heaps.—This method can hardly be applied except to ores of the 1st class, rich in sulphur and poor in copper. It is often called the "Rio Tinto" process,

though it did not originate there, and is not the only important process there used. The object of the process is to facilitate the oxidation of both sulphur and copper, allowing the excess of oxidised sulphur to pass away as vapour, and producing soluble copper sulphate—with of course much iron sulphate—at the same time.

The same effect could, of course, be produced more quickly by roasting in kilns or "burners," and this plan is occasionally adopted when time is of importance—and particularly when the excess of sulphur is to be utilised—but these conditions are rarely existent at the mines, and in most cases calcination in heaps is cheaper and requires far less plant and far less skill. At Rio Tinto the ores as raised from the mine are built up into heaps (teleras) which have gradually increased from about 100 tons up to 800, 1,000 and in some instances as much as 1,500* tons with progressive advantage. As regards cost of building and consumption of fuel, the plan adopted is to build rude flues of loose stones on the ground. These are made from 15 to 18 inches high and 12 to 15 inches wide, extending from the outside to the chimneys, which are similarly built at their intersections. The cover-stones having been loosely laid over the flues, except at their intersections, pyrites in lumps as large as a cocoa-nut or even larger is then carefully piled around and the telera is built up to the required dimensions, preferably of fragments a little larger than road

bustion is controlled by the attendants. The outer portions of the heap are often broken down and carried away for washing while still warm, and before the interior of the heap is fully burnt, especially when ore is wanted for the washing tanks, and indeed there is some advantage in washing it while warm in many cases. The whole cost of this calcination at Rio Tinto, including all labour in building and in breaking down, as well as the cost of fuel, does not exceed from 1s. 3d. to 2s. per ton of crude ore.

Washing.—When the calcination is completed the heaps are broken down and removed, any unburnt portions are set aside for re-burning, while the bulk is washed with water, sometimes by causing a small stream to percolate through the mass, but preferably by placing it in tanks and allowing the water to act upon it for a considerable time. In this way it is found that, from ores which do not contain more than 2½ per cent. of copper, rather more than three-fifths of the copper can be at once dissolved out, and most of the remainder can be got out little by little by subsequent washings at intervals of a year or so, during which air-oxidation proceeds. A good method of washing burnt ore in tanks is that employed in Spain for many years. The ore is loaded while still warm into side tip waggons holding two tons or more, and from these it is tipped direct into the tanks, which are constructed of rough masonry and cemented or asphalted inside. At Rio Tinto they were formerly made about 2 feet deep, from 10 to 14 feet wide, and from 30 feet upwards in length. They are now preferred 3 feet deep and not over 8 feet wide, which is found more effective in working and cheaper. They have a false bottom of rough planks, between which the copper liquors make their way to small chambers in the corners provided with exit pegs at different levels. The tanks, being charged with ore, are then filled with water, and this is left on the ore for 24 hours before it is drawn off. A second, a third, and often as many as eight to ten separate washings are given to the ore—the later ones being effected much more rapidly, generally at 12-hour intervals, so that the whole washing is completed in about

a week or eight days. The washed ore (*limpia de pilones* or *descarga)* is again loaded into waggons and taken away to the *terrero,* or waste heap. The effect of this calcination and washing at Rio Tinto is well shown in the accompanying table :—

TABLE III.

ANALYSES OF ORES TREATED IN THE YEAR 1882 AT RIO TINTO.

	Calcination mineral as made into teleras.	Do. after calcination as placed in washing tanks.	Do. after washing & draining.
Sulphur	48·0	6·7	7·5
Iron	41·0	39·0	55·0
Copper	2·2	2·4	1·0
Lead	2·3	2·8	3·0
Zinc	1·5	1·6	1·0
Arsenic	1·0	·5	·2
Moisture	·2	13·0	4·5
Oxygen and loss	1·3	31·0	25·3
Silica and insol.	2·5	3·0	2·5
	100·0	100·0	100·0

The liquor flowing from the washing tanks varies, of course, very much in strength at different times, according to whether it is the first or a later washing. Of course, the last washings of all are very weak, and, whenever it is possible, these are used for washing fresh batches of ore. There being a very large number of washing tanks, a brown "ferric" liquor is obtained by mixing the various streams which differs very little from day to day, some-

Ferric Liquor Process." The latter is invariably the most advantageous course to adopt, as not only does it cause a great saving of iron in precipitation, but greatly increases the yield of copper from the raw ores themselves. Pumping of strong liquors is, however, to be avoided as far as possible, and it is preferable to raise the washed ore up an incline to the waste heaps, where the natural levels are not convenient.

The Maidenpec Process.—This was for some years employed at Maidenpec, in Servia; it may be regarded as the Rio Tinto process modified, so as to deal with ores containing much more silica and much less sulphur, and which are mostly in the condition of smalls. Such ores could not be roasted without preparation in large heaps, as at Rio Tinto, but it was found that they could be pressed into briquettes and left to dry and harden, after which they could be effectually burned in heaps of about 300 tons, which, however, required much more fuel to start the combustion than the Rio Tinto ore, and burnt much more slowly.

The burnt ore was washed in tanks, and precipitated with cast iron, special precautions were taken to obtain a good precipitate (92 % copper) by working the solutions at a temperature below 65° F., keeping the tanks covered, and stopping the precipitation before the solutions were fully exhausted. The spent solutions were used over and over again for washing the "roast," thus avoiding a certain loss of copper, but necessitating an occasional "regeneration" by blowing in air, which precipitated a large quantity of peroxide of iron (ochre), sufficient to pay for the air-blast. The cost of plant and erections for treating 10,000 tons of ore per annum is given at £4,000, the wood, stone, and lime being supplied from the Company's property. The cost of treating the ore is given as follows:—

	s.	d.
Mining cost per ton	2	10
Transport	1	10
Reduction charges	1	11
Administration	2	0
Total	8	7

These figures are certainly very low, labour being very cheap in Servia. Reckoning on obtaining 1¼ per cent. of copper (from ore averaging about 2 per cent.), the cost of the copper is about £35 per ton. Ore of 2½ per cent. yielding about 1½ per cent. direct, and the remainder at the rate of $\frac{1}{10}$th of 1 per cent. per annum, can be worked to yield copper at about the same cost.* No particulars are given of the quantity of water used, of the strength of the solutions, or of the consumption of iron.

Ferric Liquor Processes.—The open-air calcination process would, no doubt, have continued to be the only one used for the ordinary Rio Tinto ores, but for the smoke nuisance, which at last became so great, that no further extension of the process was allowed. As it is, probably not less than 1,000 tons of ore per day are oxidised with an average loss of 30 per cent. of sulphur —thus 300 tons of sulphur or 600 tons of sulphurous acid, pass into the air daily, destroying the vegetation for miles around and rendering life a burden, not only to many of the people employed at the mines, but also to the villagers, in some cases as much as 20 miles away. Already in 1878, open-air calcination had been forbidden at the mines of Santo Domingo in Portugal, and a term (often since extended) had been placed on its continuance at Rio Tinto. At any rate, it was obviously out of the

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through them; part of the copper in the said raw ores being thereby rendered soluble. The same result is obtained if the washed burnt mineral itself is mixed with raw smalls, and both are subsequently washed with water. This has been called the "Ferric Sulphate" process.*

The Joly, or Doetsch, Process, patented by M. Joly about 20 years since, and afterwards known as the first Doetsch process, may be regarded as a modification of that just described. To produce what was supposed to be a very effective ferric liquor without the use of burnt ore several modes were adopted, as follows:—

(1.) Sulphate of iron was heated in a reverberatory furnace, and the "ferric salt" so produced was mixed with the raw mineral before it was lixiviated.

(2.) Sulphate of iron was mixed with salt and similarly heated, the fumes produced being carried into a condensing tower, through which "spent liquor" from copper precipitating tanks was made to fall, thus producing a "regenerated liquor," which was used for washing raw ore.

(3.) The sulphate of iron was similarly mixed with peroxide of manganese, and the fumes were similarly employed in producing a "regenerated liquor."

In modifications 2 and 3, the vapours were supposed to contain free chlorine, which, as well as ferric chloride, has a very energetic action on cupreous pyrites. This was, of course, not the case with No. 2, though large quantities of hydrochloric acid were present. In No. 3, some chlorine was produced at certain stages of the heating, but only to a very small extent.

How little effective this process in its various modifications proved itself at Rio Tinto may be seen by the fact that the great "Estacion" heap, after three years' washing, still contained more than half its copper in its original

* "My experiments have conclusively proved that a better result is obtained when burnt ore and raw smalls are mixed and washed together, than when the liquors from the burnt ore are used to wash the crude smalls. When I left Rio Tinto in 1888, the mixed washing was was being more and more extended."—H. F. Collins *(private communication).*

insoluble state. Still, the copper so liberated was obtained very cheaply, and without the serious inconvenience of the sulphur smoke from open air calcination, which, as already stated, could not be extended however much it was desired to increase the scale of operations at the mines. Moreover, the copper remaining in the ore is still being gradually oxidised and washed out, and is thus obtained at little expense.

The so-called Doetsch process, as now very extensively employed at Rio Tinto, is little more than a combined "air salt" and "ferric liquor" process, and the immense heaps of partially exhausted ores thus form valuable reserves.*

Ordinarily the liquors from such heaps are less concentrated than are those coming direct from newly-burnt ore, yet by carefully localising and regulating the water supply and causing the same waters to flow through a considerable column of ore, very strong liquors may be obtained, as may be seen from the following examples:—

1882.	Grains of Copper per gallon.
Station heap—average outflow for five and a-half months	120
Los Planes heap—average outflow for five and a-half months	301
Aug. 19.—Los Planes heap—average outflow for twenty-four hours	486

[redacted]

(*d.*) There must be a considerable and often a large supply of water available.

(*e.*) The copper must not be mainly in the condition of chalcopyrite or other "rich" sulphide.

(*f.*) It must not consist entirely or even mainly of fines.

In many cases some of these conditions are lacking, when recourse is had to some form of kiln-roasting, and kilns are sometimes used even when none are lacking, with the object of utilising the sulphur or some other product that may happen to be present.

Kiln-roasting.—When the ore is burnt in open stalls or in kilns, the partial oxidation of the copper and the removal of the sulphur are effected much more speedily, the ore being in the kiln only a few days, or even, in some instances, for a few hours, as in making sulphuric acid. The sulphur is even more completely removed, but the proportion of soluble copper is somewhat less in the first instance, though a short exposure of the roasted ore to air and moisture soon equalises the product in this respect.*

Furnace-roasting.—When furnaces are employed without the addition of chemicals they are only suited in general for the treatment of sulphide ores, but by using chemicals (usually salt) furnace treatment may be applied to all kinds of ores if the conditions are otherwise suitable, and not too expensive, with the further advantage that, not only the sulphur, but other bye-products, and particularly small quantities of the precious metals, may be utilised.

It would be impossible, within the reasonable limits of a paper for our *Transactions,* to describe the many plans which have been adopted for the utilisation of such low-grade sulphide ores as are not suitable for the simple processes already referred to. I must, therefore, limit my remarks to those which are most suggestive in theory, or have proved most successful in practice.

Bankart's Process (1845).—This is, in theory, the

* Many of the ordinary forms of kiln are well illustrated in Lunge's well-known work on sulphuric acid making.

simplest of all; in practice, perhaps, the most difficult for most ores; and I am not aware that it has ever been worked with success on a commercial scale. The ore, after fine crushing, so as to pass through a sieve of four holes to the inch, is most carefully roasted, with free access of air and frequent stirring in a reverberatory furnace, so as to convert the sulphides of copper into sulphate, while the iron is peroxidised. The roast is then lixiviated to remove the soluble copper-sulphate, part of the residue is mixed with a charge of fresh ore, and the calcination is repeated. The solution of copper is precipitated as usual with iron, and, as might be expected, it yields a fine quality of precipitate, with a very moderate consumption of iron.

It is obvious, however, that the process can only be applied to sulphide ores, and also that it must be somewhat costly, like most reverberatory work, and therefore it could rarely be applied to a really low-grade ore unless there were bye-products of value. It has now, however, been altogether superseded by the Longmaid process and its modifications.

The Linz Process, which was adopted for many years at Linz, on the Rhine, may be regarded as another form of the Bankart's process. The ore was roasted, crushed to pass through a sieve of four holes to the linear inch, and again heated. It was then treated with sulphurous acid and steam (obtained from the roasting of blende), by which

process may be employed for chalcopyrite ores containing 90 per cent. or more of siliceous matter if they are previously crushed so as to pass through a sieve of two or more holes to the linear inch. This process has formed the starting-point for a long series of modifications, as it is peculiarly suited for the utilization of ores of copper containing, besides gold and silver, small proportions of arsenic and tin, whether sulphurous or not; it was, indeed, invented for the express purpose of treating such ores. I shall describe it as it was used at the New Consols Mines in East Cornwall about the year 1878. Here, the ore, besides large quantities of sulphur, iron, and silica, contained on an average 1½ per cent. of copper and 9 per cent. of arsenic, with 9 lb. of tin, 5 ounces of silver, and a few grains of gold in each ton. With ore such as this the method required two roastings—the first, to oxidize and drive off the arsenic and most of the sulphur, was done either in ordinary reverberatory furnaces or Oxland calciners. The ore was then mixed with 10 or 12 per cent. of salt and roasted in a reverberatory furnace. By this means any sulphur remaining was converted into sulphate of soda, while the copper was converted into chloride, which was readily dissolved out for precipitation.

Any arsenic present was driven off in the first roasting, and intercepted in flues or chambers, and the tin was separated by dressing the residues after the copper had been removed in solution. The solutions were made very strong by proper tank washings; they contained most of the gold and silver of the ore, which were precipitated by well-known methods before the copper itself.

Peña del hierro Process.—At this mine, which is near Rio Tinto, a process was adopted in 1883 which may be regarded as a combination of the Longmaid and Bankart processes. It was designed to utilise the lead as well as the silver and sulphur in the ore, but was found very difficult to manage, and was soon abandoned. The ore was broken to about a half-inch size, and then dead-roasted in an Oxland's calciner, a little fuel being used in the furnace at the lower end.

The hot ore was then placed in tanks, washed with hot dilute sulphurous and sulphuric acids (made by condensing the fumes from the calciner), a little salt being added to convert the silver and lead present into chlorides. From the liquor so produced the silver was precipitated by means of copper, the copper by iron, and the waste liquor used for washing more burnt ore, after due dilution with water. As might be supposed, it was very difficult to prevent slagging in the calciner.*

The Hunt and Douglas Processes.—In that patented in 1869, the roasted ores were treated with a hot solution of ferrous chloride and sodic chloride, so bringing the copper into solution as a mixture of cuprous and cupric chlorides, the former being readily soluble in the salt, though nearly insoluble in water. From this solution the copper was precipitated with metallic iron, thereby reproducing the ferrous chloride and regenerating the solvent—"the neutral solvent does not dissolve either arsenic or antimony." This method is not adapted for argentiferous pyrites, "because the silver is with difficulty separated from the chlorides of copper."

By the process of 1881, the roasted ores are treated with a hot solution of ferrous chloride, and the cupric liquor so obtained is either reduced while still hot with sulphurous acid, so precipitating the copper as cuprous chloride; or else it is passed through a layer of copper

definitely, provided chlorine be supplied each time by the addition of a sufficient amount of some soluble chloride. I am not aware whether this process is still in use, but it was employed at Phœnixville near Philadelphia in 1883, and was also in use more recently at the Copper Queen Mine in Arizona for treating comparatively low-grade ores. I am not sure whether it is still in use there.

Many other plans have been proposed for getting the copper into solution for subsequent precipitation, but they are for the most part too complicated for use, except where the services of skilled chemists are available.

Section VI.—Copper Precipitation.

The processes of oxidation and solution just described are necessary to bring the copper into solution, but the metal has no commercial value until it has been precipitated. This may be effected in many ways, but considerations of economy practically limit us to three—viz., with iron as metal, with lime as carbonate, and with sulphurretted hydrogen as sulphide. It may also be done electrolytically, but this latter process, however has not yet become a commercial success.

Copper Precipitation with Iron as Metal.—According to theory, 56 parts of iron should precipitate 63·5 parts of copper, or nearly as 8 to 9, but this result is never attained in practice, because of the free acids or ferric salts which are nearly always present, and which consume iron unprofitably. Furthermore, very weak solutions oxidise the iron by means of the relatively large proportions of free oxygen present in them, and an improper arrangement of the iron in the tanks by which it is left exposed to the air is also a frequent source of loss, although this may be lessened by keeping the tanks covered, or, at any rate, by keeping the iron covered. All these causes tend also to the production of a low-grade precipitate.

Consumption of Iron.—The following examples from actual practice may be studied with advantage.

TABLE IV.

RATIO OF (CRUDE) IRON CONSUMED TO COPPER PRODUCED.*

	Iron.	Copper.
Mona Mines (1832-66)	1 ...	·0993
Lucencia	1 ...	·1250
Parys Mountain (1862-66)	1 ...	·1326
Cronebane (1862)	1 ...	·1400
Great Adit, Cornwall (1854-68) ...	1 ...	·1665
Connoree (1838-9)	1 ...	·2100
Wheal Agar	1 ...	·2438
Wheal Maria (1866)	1 ...	·2500
Wheal Josiah (1866)	1 ...	·2565
Santiago (1844)	1 ...	·4500
†Rio Tinto (1879-1883)	1 ...	·6580

By comparing the figures given above with the figures already given in table II. page 15, it will be seen that in a general way the most useful effect is produced when the stronger liquors are used.

It will also be seen that although the Rio Tinto practice is far the best, yet even then a great loss of iron takes place, even after allowing for the 8 to 10 per cent. average of impurities in the pig, for the ratio of useful effect is but ·715 copper to 1 of iron, instead of 1·1125 to 1.

Character of the Precipitate.—The "produce" or percentage of the precipitate is also of importance, and it is closely connected with the consumption of iron, though it is

Wheal Agar	about	33·0	per cent.	Copper.
* Cronebane, 1862	,,	35·0	,,	,,
* Ballymurtagh, 1857-61	,,	42·1	,,	,,
* Connoree, 1861-62	,,	44·0	,,	,,
* Great Adit, 1863-68	,,	46·5	,,	,
* Ballymurtagh, 1852-56	,,	46·3	,,	,,
* Santiago, 1844	,,	45·0	,,	,
Wheal Josiah, 1866	,,	46·4	,,	,,
Wheal Maria, 1866	,,	50·0	,,	,,
† Cobre Mines, 1864-66	,,	61·0	,,	,,
* Cobre Mines, 1854-56	,,	67·0	,,	,,
† Rio Tinto, 1883, 1st class	,,	92·5	,,	,,
,, 1884-86, 1st class, washed with acid	,,	95·0	,,	,,
,, 1883, 1st class, washed with acid (special sample)	,,	98·2	,,	,,
,, 1883, 2nd class (afterwards 78 °/₀)	,,	74·3	,,	,,
,, 1883, 3rd class (afterwards always over 50 °/₀)	,,	43·2	,,	,,
,, 1883, papucha	,,	24·4	,,	,,

The elimination of impurities from the precipitates is of the nature of a refining operation and will rarely attract much attention except in very large establishments.

Quantities of Water Used.—Reference has been made to the greatly varying copper contents of different natural and artificial liquors. Except in a few special cases the consumption of water is always large, and sometimes very large. Thus at Rio Tinto in the year 1882, I found that in the production of each ton of precipitated copper, not less than 600 tons of water had been used, or about 10 tons of water for each ton of ore treated. This, however, is only one quarter the quantity used in the mechanical treatment at the Atlantic Mine, though it necessitated a water storage near the mines of nearly six millions of cubic metres, which has since been increased to nearly ten millions.

Of course, when the ore heaps cover large areas in the open, the loss from sudden floods may be very great at times,

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* These are mostly from HENWOOD, *Op. cit.*

† These are from my note-book. The "papucha," or mud, was obtained by washing the regular precipitate, and was afterwards dried and calcined in open heaps and sent to the blast furnace.

unless provision is made for intercepting the drainage from the heaps, and it is rarely possible to do this completely. I have known more than 20 tons of fine copper to be thus lost at Rio Tinto in a single night.* When methods of kiln or furnace-roasting are adopted, with washing in tanks and partial regeneration of the liquors as at Maidenpec, the ore treatment is far more under control, and the quantity of water used is probably less than is required for any mechanical concentration yet successfully carried out. The proportionate loss of copper is also far less in a well-conducted wet process.

General Conclusions on Iron Precipitation Methods.—On the whole, it appears that the following general conclusions are fairly justified :—

(*a.*) Strong liquors give better precipitates than weak ones.

(*b.*) A high temperature is better than a low one as regards the rate of precipitation, though not as regards the quality of the precipitate formed from an impure liquor.

(*c.*) The precipitation is more rapid in a flowing stream than in a still tank.

(*d.*) Pig-iron is more advantageously used than wrought-iron scrap (and especially than tin scrap) when both have to be purchased.

Electro Deposition.—The solutions for this purpose may either be obtained (1) direct from the ores, or (2) from slabs

more distinctly a refining process in the technical sense, and, therefore, outside the intended scope of this Paper.*

Precipitation as Carbonate and Hydrate.—This method is not to be recommended except where the precipitate is required for fluxing operations in smelting. The precipitate must generally be very poor, owing to the large amounts of sulphate of lime and of carbonate or peroxide of iron, which are simultaneously produced. Still, where smelting operations are carried on, it is sometimes well to adopt this method if lime can be cheaply obtained. This is best applied as a sort of paste of milk of lime.

Precipitation with lime was adopted at Wheal Falmouth about the year 1830, when a precipitate of about 10 per cent. copper was obtained, which at that time realised from £8 to £9 per ton.†

Precipitation as Sulphide.—Sinding's process, in which the copper is precipitated with a crude sulphuretted hydrogen, produced by the inter-action of carburetted hydrogen, sulphurous acid, and steam at a high temperature, was first employed at Földal, in Norway, in 1849,‡ and subsequently at Swansea, Rio Tinto, and many other places under the name of the Hartmann process. At Földal carburetted hydrogen gases and steam were produced from sawdust and half-burnt wood by the aid of a blast in a separate furnace or generator, and then made to pass over burning pyrites while the access of air was carefully limited. In this way sulphuretted

* Arrangements are now far advanced for the electrolytic treatment of the rich Montana mattes on a great scale, but the matte is first converted into metallic copper by the Manhès process. Moreover, the original ores are by no means of low grade, those of the Boston and Montana Company averaging over 9 per cent., besides being rich in silver. The electro-refining of pig copper has been carried on for many years at Röraas, in Norway, where very poor pyritous ores are smelted to matte, and reduced to pig copper by the Manhès process in a Bersemer convertor.

† A similar precipitate at present would hardly realise more than £3 5s. per ton.

‡ It was first devised by Sinding and Stromeyer in 1847 as a method of obtaining sulphur.—See *Norske Ertsforekomster* III. Part VII., 1889, p. 59.

hydrogen and carbonic acid were produced, which, being led into the solutions obtained by washing the burnt ore, precipitated the copper as sulphide, the iron present remaining in the solution owing to its acidity.

When this process was employed at Rio Tinto in the year 1882 coal was used in the generator, instead of charcoal and sawdust. The air-dried precipitate averaged a little over 10 per cent. of copper, which was raised to 64 per cent. by calcination. The calcined product was sent to the blast furnace with other rich products. The precipitation of copper as cuprous chloride in the Hunt and Douglas process has been already sufficiently referred to.

Section VII.—Raw Smelting Methods.

General Conditions.—Under the best circumstances of fusible ore and cheap fuel, these methods can hardly be so cheap as the "wet" methods referred to in the preceding sections, so that, when copper alone is in question, they are rarely applicable for really low grade ores.

But such ores are often relatively, though not actually, rich in the precious metals, and, moreover, they are frequently associated with other substances of value. In such cases it is sometimes found that the copper itself may be better concentrated, and the other substances referred to more readily utilised for subsequent refining

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though oxidised ores may be in some cases directly converted into black copper of 94 to 96 per cent.

Calcination.—Sulphides, of course, need to be first calcined; other ores need no preparation, except to see that they are so mixed as to yield a sufficiently fusible and fluid slag, though in most cases fines have to be made into lumps or briquettes. At Rio Tinto the richer and more siliceous minerals (mineral cuarzo), instead of being crushed and mechanically concentrated as they would be in mines where no chemical processes are employed, are made into small teleras of 200 to 300 tons and roasted in the open. The method of construction is somewhat different to that employed for the ordinary calcination mineral, and much more fuel is required. The ground chosen is covered to a depth of 2 feet with brushwood, and several bundles are placed in a sheaf in the centre to form a chimney when burnt out. Ore is then piled around and the wood is kindled, no layer of smalls being placed on the outside as in the ordinary heap-calcination. Such a telera will continue to burn for about three months.*

Heap-roasting can generally be effected, more or less satisfactorily, when ores contain not less than from 15 to 20 per cent. of sulphur, while stall-roasting and kiln-roasting can often be usefully worked when there is not more than 10 per cent. The cost of heap-roasting varies from 1s. 6d. up to 3s. 6d. per ton, including the building and breaking down of the heaps. Stall and kiln-roasting are usually a little more costly, but the process is more under control in case of bad weather. It is also more rapidly completed, so that far less capital is locked up at any one time, and the burnt mineral is less subject to loss from sudden floods—a serious matter in some countries.†

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* Of late years, at Rio Tinto, the calcination of rich mineral has been conducted in teleras of the same size and shape as those of ordinary calcination ore, only it has been allowed to burn a little faster.

† That this is no merely imaginary danger is shown by the fact that at Rio Tinto in the year 1881 the copper liquors were produced in

Kernel-roasting.—This peculiar concentration of copper, which has often been described, takes place, to some extent, in almost all calcination processes when low-grade ores are burnt without free access of air; even balls of fines yielding kernels in some instances. At Rio Tinto a watch is kept for these kernels in breaking down the teleras, and removing the burnt ore for washing. The kernels are afterwards smelted to matt in the blast furnace.

This process was used at Földal, in Norway, as early as the year 1770, and was the main process in use at that time from 1807 to 1848. The ore treated was a pyrites, containing from 45 to 48 per cent. of sulphur, with 2 to 2½ per cent. of copper, and 3 to 6 per cent. insoluble matter. The ore was burned in heaps in the open air as slowly as possible, from three to four months being usually required to burn a heap. After roasting, the kernels, containing only about 3¾ per cent. copper, were at first—and, indeed, for many years—picked out by hand, and at once smelted into matte. It was afterwards found that, by first washing the burnt ore, so as to extract the soluble sulphates, the kernels could be much more easily picked out from the dried ore: and, as they carried much less of the adhering oxide scale, their assay rose to 12 per cent., and even to 15½ per cent.

The proportion of copper obtained in the kernels was small, only one-sixth or even one-seventh of the total

ployed, the raw smalls being moulded into balls with a little clay. When the process was completed the outer portions or "shells" were removed and washed, thus yielding a copper liquor for precipitation, while the kernels were subsequently collected for smelting.* A similar process has, for a long time, been carried on in Japan.

Attempts have been made on many occasions to separate the kernels from the shells by jigging, but without much success, owing to the slimy character of a large proportion of the shell material as soon as it is wetted, and the powerful corrosive action of the liquor on the jig-sieves. As an illustration of the concentrating effect of kernel-roasting on ores averaging between 2 and 3 per cent., the following examples from Rio Tinto may be given:—

(*a.*) 88·3 per cent. of shell-residue reserved for jigging contained 1·13 per cent. of copper, and yielded 1·17 per cent. of kernels containing 5·46 per cent. of copper. The solution obtained in jigging was not separately analysed. This systematic jigging was not sufficiently effective; moreover, it was soon found to be quite out of the question on the enormous scale necessary for the work at Rio Tinto, and was abandoned for the old process of hand-picking before washing in tanks, which is still very extensively in use.

(*b.*) 177 tons of kernels specially selected by hand-picking from February 23 to April 8, 1880, averaged a little under 15 per cent.

Roasting of Fines in the Reverberatory Furnace.—This is a very effective process, but the cost has usually been considerably more than that of heap or kiln-roasting. The old-fashioned brick calciner, from 50 to 60 feet long, in which the ore is gradually worked forward by two attendants in each shift, has been pretty well superseded by the automatic Brückner and O'Hara furnaces in America, and generally where labour is dear. These could hardly be said to have cheapened the cost of

* See *Rep. Min. Assoc. of Corn. and Dev.*, 1869, p. 11, &c.

calcination, but the new annular automatic calciner of Mr. R. Pearce, as used at the Argo Works in Denver, has rendered it possible to calcine highly pyritous smalls at a total cost of about 2s. per ton.

Smelting to Matte.—This may be effected either in reverberatory furnaces, in water-jacketed cupola furnaces, or in shaft furnaces of brick or stone. The "mineral rico" and the "mineral cuarzo" selected from the calcination ores at Rio Tinto, averaging, in 1883, 7·23 per cent. and 4·17 per cent. respectively, were smelted, after roasting, together with the selected kernels from the calcination heaps and washing tanks, and certain inferior copper precipitate washings ("papucha"), in low brick furnaces lined with a soft and compact clay-slate. About 60,000 tons of these products were smelted during the year, selected from or resulting from the treatment of nearly one million tons raised from the mines, which yielded 7,000 tons of matte averaging a little over 36 per cent. of copper. I cannot give the cost of this series of operations, but they were decidedly remunerative with coke costing about £2 per ton. I believe the expense has since been lowered by the use of an improved type of furnace provided with a low water-jacket. They now smelt at Rio Tinto 300,000 tons annually, and the cost of the copper so obtained is said to be really lower than the average of the cementation of copper. In 1888 there

An instructive example of matte-smelting of low-grade silver and gold ore on a copper basis is afforded by the works carried on at Sunny Corner, in New South Wales. The ore is a complex sulphide much like the "bluestone" of Anglesea, having about the following average composition :—

Zinc	22·0	per cent.
Lead	12·0	,,
Iron	12·0	,,
Copper	1·5	,,
Arsenic	6·0	,,
Sulphur	20·0	,,
Silica and Silicates	20·0	,,
Moisture, and not determined	6·5	,,
	100·0	

Silver from 15 to 20 oz. per ton.
Gold about 3 dwts. per ton.

Of course, this would be valueless if considered as a copper ore in such a situation, and at first it was treated as a lead ore, a small quantity of rich lead being produced and the copper disregarded. But it has since been found more advantageous to volatilise the lead, zinc, and arsenic as far as possible, and to concentrate the gold and silver into a small quantity of copper-matte of 18 to 20 per cent., which is found to contain from 250 to 300 ounces of silver and nearly 3 ounces of gold to the ton. The process is as follows: The ores are first roasted in heaps of about 300 tons, a good deal of cheap brushwood being consumed in the process. The roasted ores, together with some natural gozzans and a considerable proportion of limestone flux, are then smelted in a large oval furnace, provided with a cast iron water-jacket in sections. The total cost of mining and smelting, including the freight of the matte, is said to amount to about 34s. per ton of ore.

The Smelting of Fines in a Blast Furnace.—Fine ore

should be bricked as far as possible; but when for any reason this cannot be done, it may sometimes be smelted direct in a water-jacketed cupola furnace, using a low burden and a very light blast. Of course, the fuel consumption will be rather high, and the labour cost will be somewhat higher than when lump ore of similar composition is smelted, owing to the slower rate of smelting which is always experienced. The bricking of fines can sometimes be affected by simply mixing them while moist with flue-dust or ore-slimes, pressing, and drying; but sometimes a mixture of lime, or even of Roman cement made from precipitated sewage, is admissible.

But Mr. Peters has shown that even green (unroasted) fines can be smelted in a blast furnace of large section using a "fan-blast." Of course, the furnace yield is low, but the cost, with cheap fuel, is only about 8s. per ton.*

Smelting to Black Copper.—The smelting of completely oxidised ores to black copper cannot be effected so cheaply as the matte-smelting of sulphides. Still, if the ores are basic enough to form their own slag, they may be smelted into bars of over 94 per cent. in circular water-jacketed furnaces 36″ diameter at the tuyeres at a rate of 40 tons per day, at a cost not exceeding 12s. 6d. per ton of ore, when coke can be got at about £1 per ton. Large water-jacket furnaces may be used with still greater advan-

there is also present either gold or silver, or both. But the addition of a comparatively small proportion of an ore rich in sulphur, say enough to yield a copper matte of 40 to 50 per cent. instead of bars of 94, will much cheapen the working and give a larger copper product, as the slags will be very much cleaner, though the advantage of this may be neutralised in the less advantageous sale of the product.

The Hollway Process.—This is an application of the Bessemer process to sulphide ores, by which they may be smelted practically without the use of fuel. Many interesting experiments on the Rio Tinto pyrites were made at Sheffield in the years 1878-9, the results of which were discussed before the Society of Arts, and reported in the *Journal.* These experiments were made with improvised and not altogether suitable apparatus, but the results, on the whole, were regarded by many practical men as highly successful, inasmuch as pyrites containing only 2½ per cent. of copper was concentrated into a matte of 15 per cent., while a certain proportion of the sulphur was separated in the free state. In these experiments coal was used for the production of the blast, and coke for the first melting of the pyrites; but the fuel for the concentration and enrichment of the copper matte was supplied by the combustion of part of the sulphur contained in the ore. Some crude but interesting experiments were afterwards made at Rio Tinto by my friend, Francis Bawden, in the year 1882, using the low slate-lined furnace already referred to. Tuyeres of 2½ diameter were employed, and a cold blast. The furnace, once started in the ordinary way, ran without coke fuel for 24 hours, the tuyeres dipping into the melted pyrites. Much free sulphur was given off, as in the Sheffield experiments. The charge was 2 parts of crude and 3 parts of calcined "mineral cuarzo," of 6 per cent. and 7 per cent. copper respectively. The silica and aluminous silicates probably amounted to about 30 per cent. of the whole charge, and the product was a matte of 25 per cent. copper and a slag of 0·5 per cent., with a sp. gr. of 3·6.

Under the circumstances, this must be regarded as a very good result. It could have been still improved by the use of more and smaller tuyeres, and I have always thought it a great pity that the Hollway process was not adopted on a large scale at Rio Tinto, as it would, in my opinion, have largely increased the profits besides practically doing away with the smoke nuisance. It may, however, be said that it has been most successfully adopted in the form of the now well-known Manhès process for the enrichment of copper mattes.

A complete and extensive Hollway plant was erected in 1884 for the treatment of the Maidenpec ores in Servia. I have not heard how far it was successful there, but the ores being much less sulphurous were certainly not so well adapted for its use as the Spanish ores.

Some very similar experiments were made by Mr. Claude Vautin at the Great Cobar Mines in Australia in the year 1886.

Section VIII.—Concluding Remarks.

Scale of Working.—As the ores to be utilised often contain less than £1 worth of copper in each ton, and in some cases less than one-third of this amount, it is obvious that the cheapest and most effective methods alone can be employed, both in the mining and the after-

admissible for those belonging to the second class (the oxidised ores), nor, so far as is proved at present, in places where water is scarce, nor where the vein-stones are of high specific gravity.

Wet processes cannot be applied unless there is either an abundance of sulphur in the ore, or where cheap (waste) acid is obtainable. And not always even then. In most cases large quantities of water are also required. The cost of iron and that of fuel for subsidiary use must also be considered.

Smelting processes require at least that fuel shall be moderately cheap, and if flux is needed that it should be very cheap indeed. When water-jacket furnaces are used there must be a considerable water-supply, especially if the climate is hot, though such is not required if other forms of furnace can be used.

First-rate transit facilities for the necessary machinery and supplies, and for the distribution of the products, although not perhaps in every case essential, are yet of very great importance, since the products are at best only of moderate value. When wood is abundant they are, perhaps, less essential in the case of smelting than in the other processes.

I may add that it is rarely advantageous to work up the ores into very pure products, since refining processes are almost always far cheaper when carried on in special establishments; but, of course, the value of the product must be so raised by the method of concentration adopted that it will bear the necessary transport charges —or even reduce them to comparative insignificance.

Samples, Assays, and Accounts.—In the treatment of low-grade ores by any other than a mechanical process, skilled sampling and assaying are almost constantly necessary if the best results are to be attained. In particular, the waste solutions and residues must be closely watched, continually sampled and assayed.

Similarly, the most careful and accurate accounts must be kept of each stage of the operations, especially

when they are of a complex nature. In this way it may be ascertained where it will be advantageous to save and where to sacrifice a little more of any particular product, or even when one or more may be sacrificed altogether with advantage.

The Utilisation of Bye-Products.—As already intimated, many low-grade copper ores contain small quantities of valuable substances. Such bye-products are almost always present in greater or less quantity, and it will often happen that some, at least, may be profitably concentrated and utilised by a proper sequence of operations. It is here, indeed, that the functions of the miner and metallurgist often overlap, and even coalesce, for, in many cases, the operations must either be conducted at the mines or not at all; and it may not be possible to have the services of separately skilled superintendents, at any rate, in the early stages of an enterprise.

The chief bye-products with which we are here concerned, are the metalloids—sulphur and arsenic; the metals —gold, silver, lead, nickel, cobalt, manganese; oxide of tin; the sulphates of iron and zinc; and the oxidised ferruginous substances known as purple-ore or blue-billy, and ochre. It must often be advantageous to sacrifice some of these so as to get the others more cheaply; while varying circumstances of demand may render sometimes

pyritous ores by any of the wet processes; the oxide of tin, if present, and the ochre may be obtained by a mechanical treatment of similar residues.

The consideration of the different processes necessary for such varied aims is scarcely more within the proper scope of the present paper than the subsequent refining operations. They will be best dealt with in an entirely separate paper.*

General Conclusions. — In conclusion, I would remark:—

(*a*) That whenever an extensive deposit of low-grade copper ore is met with, it is deserving of the most careful consideration as to its components and its working facilities, whether alone or in combination with other accessible ores.

(*b*) No metalliferous residue, whether of ore-dressing or metallurgical operations, if it is produced in considerable quantity, ought to be finally disposed of until its components have been thoroughly ascertained by appropriate chemical or other means.

(*c*) In the treatment of low-grade copper ores it is rarely advisable, and perhaps hardly possible, with advantage, to separate the work of the miner from that of the metallurgist.

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* Of course, it does not follow that because substances of intrinsic value are passing away in the residues, that the processes are to be condemned as inefficient. It does not pay to save a sovereign at the cost of a guinea, though it may be well to get it for 19s. 6d. But it may serve to give a useful idea of the magnitude of the operations at some great mines, and of the field still remaining for invention and discovery, if I here remark that there is mined in the Sierra Morena every year, and finally lost, about 400,000 tons of sulphur (as sulphurous acid, and, in combination with iron, zinc, and other metals, as sulphates), 1,500,000 tons of iron, 16,000 tons of lead, 20,000 tons of arsenic, 12,000 tons of zinc, 15 or 16 tons of silver, and 1 ton of gold, besides other substances of value, such as nickel, cobalt, manganese, thallium, and selenium.

The sulphurous acid is positively destructive to the agriculture of many square miles of country, and more or less injurious to the health of at least 30,000 people.

(*d*) The constant services of a skilled chemist are necessary in connexion with the simplest wet process if the ores are subject to much variation, if the best possible results are looked for, and especially if the sulphur, precious metals, and other bye-products are to be utilised to the fullest extent.

DISCUSSION.

Mr. Percival Fowler: I think we should only be doing our duty if we, while thanking Mr. Collins for his valuable paper, took steps to have it published for the benefit of all those connected with mining, and more especially with the subject which he has treated. I therefore suggest, before making any remarks this evening, that the paper should be printed in full, and not as Mr. Collins has read it, because he has been kind enough to save our time by cutting it very short, in order that discussion might follow. On the many points raised, there is one which I should like to have heard Mr. Collins refer to more particularly, and that is what actually happens during the precipitation of copper, as, of course, we have to get the scientific point of view if we want to save money in the practical working. I do not know if you would mind my putting on the blackboard some formulæ which I have

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Mr. J. H. Collins : I have carefully avoided going into any of these scientific questions of formulæ. My paper is on the economic treatment of copper ores, and I do not think it matters much from that point of view what the reactions are. I think the usual course is for a practical man to work out a method of precipitation, and then for a scientific man to come along and dispute with some other scientific man as to what takes place. My paper had only to deal with what the practical man was doing in getting the copper out of the ore, therefore a discussion of these

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But further reactions actually take place, due to the presence of the iron, the oxygen contained in the air and waters, and the ferric sulphate contained in the waters under treatment, thus :—

(2.) $Fe_2O_3\ 3SO_3 + Fe + O = FeOSO_3 + Fe_2O_3\ 2SO_3$.
(Ferric Sulphate.) (Iron.) (Oxygen.) (Ferrous Sulphate.) (Ferric Sub-Sulphate.)

This subsalt of iron formed is called the First Papucha, and has a tendency to precipitate, which it does, if the waters are highly charged.

In addition to the above subsalt, if the waters are highly charged and excess of iron, as is usually the case, be present, a further reaction takes place, thus :—

(3.) $Fe_2O_3\ 2SO_3 + Fe + O = FeOSO_3 + Fe_2O_3SO_3$.
(First Papucha.) (Iron.) (Oxygen.) (Ferrous Sulphate.) (Second Papucha.)

The Second Papucha is less soluble than the First Papucha.

It will be seen, therefore, that the presence of large quantities of ferric salts in the copper waters causes the consumption of more iron than is necessary for the precipitation of the copper.

It is thus of the greatest importance, first, to avoid, as far as possible, the presence of ferric salts in the copper waters, and secondly, to keep the iron from contact with the air.

In order to diminish the amount of ferric salts present, an efficacious method adopted is to pass the waters, containing ferric salts, through fine uncalcined mineral, which tends to convert the ferric into ferrous salts, thus :—

(4.) $(Cu_2SFe_2S_3 =)\ CuS_2 + Fe_2O_3\ 3SO_3 = CuOSO_3 + 2(FeOSO_3) + 2S$.
(Iron Pyrites.) (Ferric Sulphate.) (Cupric Sulphate.) (Ferrous Sulphate.) (Sulphur.)

and—

$Cu_2O + Fe_2O_3\ 3SO_3 = 2(CuOSO_3) + 2(FeOSO_3)$.
(Red Copper Ore.) (Ferric Sulphate.) (Cupric Sulphate.) (Ferrous Sulphate.)

theoretical questions will not be discussing the paper. I say that with all deference to Mr. Fowler, whose illustrations might form a very interesting addendum to it, and will be very useful to us by and bye.

Mr. Fowler: The question is with regard to the saving of iron; as Mr. Collins pointed out, there is about 50 per cent. lost. If these formulæ are right, perhaps some of us may, through them, work in the direction of saving a further amount of iron, which is now wasted in the precipitation process.

The Chairman: I am inclined to take Mr. Fowler's view of the matter in this case. Of course, the use of formulæ should be kept within reasonable bounds, but unquestionably although many processes originated by rule of thumb, there is no reason why they should continue so. It is only by the application of scientific principles that one can see whether any *bonâ fide* improvement can be introduced.

Mr. Fowler: While I was in the Rio Tinto district I made a very careful study of the various methods adopted, which Mr. Collins has mentioned in his paper, to effect a saving of iron, and while we know much has been done in recent years, there still appears room for further saving, more especially if we know the causes at work which prevent all the iron from doing its effective duty. I may say, with regard to the paper generally, there are two or three points on which I should have liked to have had Mr.

something over 8 grammes per thousand kilos. A good deal depends upon the facilities for procuring water. I should like to know if Mr. Collins has any interesting figures to add to the paper with regard to that particular point.

Mr. Claude Vautin: I rather wish Mr. Collins had not made the remark he has as to the scientific bearing of the paper, because I wanted to ask him a question which might be considered outside the economic treatment of copper ores. In his paper, he stated that 90 odd tons of copper had been precipitated in the Carnon or Bissoe Valley, which varied from 6¼ to 60 per cent. of copper. Could Mr. Collins tell us what the balance generally consists of in these precipitates? I am sure it would be very interesting to know. While speaking of precipitation, and also referring to the methods of precipitating copper as sulphide, it struck me that all the means of precipitating copper from sulphate solutions or chloride solutions had not been exhausted. Personally, I have gone through some experiments, and have noticed that monosulphide of iron causes a complete precipitation of copper from a solution of its sulphate and also from its chloride; and it has also struck me that the iron sulphide could be easily produced by simply running down some of your low cupreous pyrites, fluxing off the gangue. Crushing the matte so produced, say to forty holes to the inch, and allowing the copper liquors to percolate through the crushed matte, nearly the whole of the iron of the matte will be replaced by copper. Perhaps Mr. Collins will say if this method has ever been tried.

I should also like to ask Mr. Collins if he can give us any information about the Austin direct method of smelting sulphides. The Austin process, from what I can hear, is a modification of the ordinary blast furnace process and the Hollway combined. In it use is made of the heat generated by the rapid oxidation of the iron and sulphur, and there is produced an iron or cupriferous matte as the case may be, and from late information I received from Colorado it is doing marvellously well; in fact, a furnace has been erected

in Wales here, but they tried to go too far and treat fines on the same principle, and naturally failed. Referring to Bessemerising of copper matte, I had rather a curious experience on that point. At Cobar, New South Wales, many years ago, we smelted, or rather melted, cupriferous iron pyrites containing 4 to 5 per cent. of copper in small blast furnaces, and then tapped them direct into the water jacket lined converters. We had one or two serious experiences as regards the linings, but we got over the difficulties, and after we had blown the charges in the converters to 30 to 40 per cent. copper, we tapped from the converters into the reverberatory furnace, and made a complete separation of the matte from the slags, which were remarkably free and clean from copper. I may, in passing, state that the water jacket lined converter gets over all the troubles which were experienced by Mr. Hollway when he first introduced that process. Next to the water jacket furnace, carbon bricks form an excellent lining. Referring to the open air methods of calcination, I do not know whether at Rio Tinto they made any special effort to obtain a complete burning or oxidation of the material of which the edges of the heaps was composed. At Sudbury they got over that difficulty. It generally happens that an incomplete calcination or oxidation on the edges of the heaps is experienced. After the heaps have been partially calcined, the space between the two heaps is then filled in with fresh ore

the percentage of sulphur left would be too high. I should very much like to know what percentage of clay was used in balling the sulphides. Mr. Collins simply states in his paper that such ores could not be roasted without preparation in large heaps. Under the head of "Furnace-calcination methods," Mr. Collins states that the copper "must not consist entirely, or even mainly, of fines." After balling or bricking, could not the furnace-calcination method be used sometimes to advantage? Then, referring to Bankart's process, converting the copper into sulphate, can Mr. Collins tell us whether, after the ore has been oxidised, the products of the decomposition taking place, we will say in No. 2 furnace, have been passed through the previously oxidised material in say No. 1, and whether there would not be a reaction between the products of decomposition from No. 2 on the oxidised ore in No. 1, whereby complete oxidation would be obtained. It is of interest that Mr. Collins has referred to the Longmaid process. I suppose the percentage of gold in any ore successfully treated by this process must be exceedingly small. Mr. Collins says, "This process has formed the starting point for a long series of modifications, as it is peculiarly suited for the utilisation of ores of copper, containing, besides gold and silver, small proportions of arsenic and tin." I do not know whether they have ever been able to obtain anywhere more than a few grains of gold to the ton by that process. Once I thought I noticed in kernel-roasting that some silver appeared to find its way to the outside of the calcining lump. Perhaps Mr. Collins can tell us whether it is a fact that silver becomes concentrated on the outside of the lumps. I do not know whether it was only fancy on my part, or an actual fact, but we certainly found silver on the outside edges of some of the pieces, and copper sulphide in the centre. As to the remarks made by Mr. Collins with reference to the application of the Bessemer principle to the Rio Tinto ore as carried out by Mr. Bawden, the use of 2½ inch tuyeres was probably the cause of any non-success. What is wanted is a very large number of small tuyeres, so arranged that they

could be removed from below the matte, should occasion require. This is rather a hasty review of Mr. Collins' exhaustive paper, but I trust, Mr. Chairman, you will take the spirit for the deed.

Mr. Benedict Kitto: The paper is so very exhaustive that one should read it over carefully before making any remarks at all upon it. A simple reading, even when performed so audibly and distinctly as Mr. Collins has done, is hardly sufficient for one to put one's ideas together upon such a very minutely and carefully written paper. I may say with regard to one of the processes mentioned by Mr. Collins for precipitating the copper—namely, the Hartmann, as used at Rio Tinto—the company made some very exhaustive and large experiments upon that method of precipitation by means of sulphuretted hydrogen, generated in the manner described in the paper, and up to a certain point it was successful—that is, up to the point of the actual precipitation which produced a very fine mud of sulphide of copper; but they found that it required so much handling, and was so messy, that really the process was practically unworkable. I believe that is the reason the very expensive plant which they erected to try it on a large scale has been removed. In regard to the process of roasting to produce a sulphate from the sulphide of copper, nine or ten years ago I made a series of experiments in a reverberatory furnace, and I got some very good results. I constantly took the temperature of

see what they are producing from the ore and what they are throwing away. Mr. Collins, having been the chief chemist there for some years, will know that the company took very great care indeed to get the best possible results out of their ore. Nor were they satisfied with their elaborate arrangements upon the spot. For some years they kept an experimental laboratory open in London, with a view to test the matter further in this country. These experiments in England have been given up for some time, but I just mention this to show that the Rio Tinto Company are working in a very careful and scientific manner so as to get the excellent results which they obtain from their low class ores.

Mr. A. L. Collins: I should like to refer to one of the many points raised by Mr. Vautin—that is, as to the concentration of silver along with copper in the kernel roasting process. Professor Vogt, who has very carefully studied the question in every way, and experimented on the point, at Foldal and elsewhere, told me that not only copper, but cobalt and nickel, and, I believe, silver also are concentrated in the kernels along with the copper; so much so that he has proposed that the process should be worked on a practical scale for the nickeliferous pyrites of Norway. I believe the concentration of the cobalt and silver is not in so great a ratio as the concentration of the copper, but it is nevertheless very distinct.

The Chairman: If no one else has anything to say just now, I will ask Mr. Collins to reply to the questions which have so far been asked.

Mr. J. H. Collins: Several of the questions put by Mr. Fowler and others must be answered by a reference to the paper itself. It was so long that I was obliged to leave out a great many details in reading it. Mr. Vautin's first question is very easily answered. When you get a very poor precipitate the bulk of what is left after extracting the copper is oxide of iron. There is a certain amount of mud, because the water is sometimes dirty, but the bulk of it consists of oxide of iron. Sometimes there are pieces of metallic iron. If tin scrap is used

there is a certain proportion of tin also; if cast iron is used there is a good deal of carbon. With regard to the use of nitric acid and nitrates in the extraction of copper from low grade copper ores, this seems quite outside the scope of the paper—a very interesting thing in the laboratory, but I do not think it can ever be practically applied on the large scale to low-grade ores. With regard to precipitating copper solutions with melted pyrites or matte, that is another question altogether. Solutions have been very largely passed through fine ore, but I am not aware of any very extensive experiments which have been made on the lines of his suggestion in the use of pyrites which had been melted and partially enriched. That is certainly a very fair subject for experiment, and, perhaps, not an unhopeful one, though I fear the precipitate produced would be rather foul. I do not know wherein the Austin process differs essentially from the Hollway process. It seems to me, from the little I have heard about it, to be very like what Mr. Bawden did at Rio Tinto. There is no doubt with regard to the Hollway treatment that one of the great difficulties has been the lining. The melted pyrites attacks almost every kind of lining very powerfully, but, as Mr. Vautin has said, a water jacket of metallic iron will resist it fairly well, and I dare say a lining of some form of crushed coke mixed with clay might be used. At any rate, it is only in the earlier

the Canadian practice, but rather the reverse. As to the Maidenpec process, I do not know much more about it than what I have said in the paper. There was clay sometimes used to brick the ore for calcining, but how much, or how much sulphur, was left after calcining I am not able to say. With regard to the Longmaid process, I think it would not be applicable to rich ores of gold, but it is very successful with low-grade siliceous ores of copper containing small quantities of gold and silver.

The query as to the situation of the silver after kernel-roasting has been answered by Mr. Arthur Collins. Mr. Fowler's formulæ are, as I have said, a little outside the scope of my paper. I have not particularly studied them, and should not like to say very much about them just now, though, of course, I have notions of my own on the subject. With regard to the first, there is no doubt about that; it is the truth, but it is not the whole truth. It would be the whole truth if the copper solution consisted simply of sulphate of copper. There are, however, a great many other substances present, and they might perhaps necessitate a complex formula instead of a simple one like that. But this whole question of the chemical formulæ of the different processes is outside the scope of my paper, which is already too long. I think it would be interesting to some of us who are chemically inclined to discuss it, and I would suggest that Mr. Fowler should bring before us a paper on the chemical reactions of these various processes. With regard to the strength of the solutions, no doubt Mr. Fowler has been correctly informed that it pays better to leave a little copper in rather than to extract the whole. We used to assay from the various canals every day samples which had been taken during the whole time—a drop at a time for 24 hours —we assayed every day the various liquors going into the tanks and the liquors going out of the tanks. They did not attempt practically to get the liquors with less than about 7 or 8 up to 20 or even 30 grammes to the cubic metre. Sometimes they were very high indeed, which showed bad work, or else an inability to compete with floods. Eight grammes to the metre is, of course, strong,

compared with the water that was successfully treated in the Carnon Valley in Cornwall. If such liquors as run away into the river at Rio Tinto could be got into the Carnon Valley in Cornwall, we should see a very large industry springing up there; but that does not imply that the work done at Rio Tinto is bad work. Mr. Kitto referred to the Hartmann process at Rio Tinto, with which I had a good deal to do, not with the process itself, but with the results of the process. It is quite true that the precipitate produced was a very troublesome one. It was very bulky, as everybody will know who has precipitated copper with sulphuretted hydrogen in the laboratory, but still I do not think that was the reason the process was given up. The precipitate, bulky as it was, might have been compressed in filter-presses, and that would have reduced its bulk very much. After that it might have been, as in fact it was, calcined until it contained 60 per cent. of copper, and was more or less sintered together. Every metallurgist will know that with a sintered product of 60 per cent. of copper there is no further difficulty. The fact is the process was an expensive one; it could not be worked at Rio Tinto as cheaply as other processes, and I think the most was made of the inconvenience of the bulky precipitate by way of excuse for giving it up. Mr. Kitto's experiments on the roasting of copper ores to a soluble sulphate, bear out the few remarks I made about Bankart's process. It is

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process has been at work now for about two years on some ore containing only 3 per cent. of copper, that nitrate of iron is used in the first instance, and that they have a method of regenerating their solution. He assured me it was a complete success, so much so that I certainly tried it in the laboratory on a small scale, and found that it acted. They have treated up to the present time some 8,000 tons of copper. If you will allow me to refer once more to what has been said about the condition of the copper precipitate as a sulphide by the application of sulphuretted hydrogen, the copper precipitate, by the use of solid crushed sulphide of iron is of course in an entirely different condition, and easily handled.

The CHAIRMAN then announced that the discussion would be continued at the next meeting.

SECOND ORDINARY MEETING.

The Second Ordinary Meeting of the Third Session took place at the Museum of Practical Geology, Jermyn Street, S.W., on the evening of Wednesday, December 20th 1893, at 8 o'clock.

Mr. George Seymour (President) occupied the chair.

The Secretary read the minutes of the last meeting, which were duly confirmed, and also announced the names of gentlemen who had been admitted to the Institution since the date of the last meeting.

The discussion on Mr. Collins' paper was then continued.

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THE ECONOMIC TREATMENT OF LOW GRADE COPPER ORES.

DISCUSSION (*Continued*).

Mr. A. G. CHARLETON: The title of Mr. Collins' paper, "The Economic Treatment of Low Grade Copper Ores," affords us, I think, many important points for consideration, and we all of us must feel grateful to Mr. Collins for the clearness and conciseness with which he has dealt with the subject. As regards its title, I would like to remark that the primary object of mining (so far, no doubt, as it concerns the consumer) is to obtain certain metals and minerals needed for carrying on the various industries and exchanges of the world at the lowest possible cost. For the mining engineer, however, who represents the producer, the chief aim of mining is, without doubt, to earn the largest possible profit on a given expenditure of capital entrusted to him. The joint operation of these laws naturally tends to cheapen the average cost of production all the world over. Since the average cost of production to a large extent regulates the market price of every metal when supply and demand balance one another, it is of the utmost possible importance, not only to the producer but equally so to the consumer, that the miner should study every possible means by which the local cost of production can be reduced; and also that he should ascertain for himself what the general average cost of producing the metal actually amounts to on a given unit, such as the lb. of copper. Papers like Mr. Collins' add certainly to our stock of valuable information on the

former subject. Mr. Collins refers, I notice, to the Atlantic Mine, which is indeed a remarkable instance of an admirably managed property, paying dividends regularly from workings becoming deeper and more extensive year by year, and on one of very low and declining yield. It appears, in fact, as Mr. Collins says, that the total yield of copper obtained per ton of 2,000 lbs. of ore milled amounted to only ·65 per cent. in 1891, compared with ·98 per cent. in 1874 and 1875. This declension in yield, it should be remarked, has been gradual, and has followed the increasing output of the mine—thus, when in 1880, 169,825 tons of ore were stamped, the yield fell to ·71 per cent.; where in 1888, 298,055 tons of ore were stamped, the yield fell to ·66 per cent. Previous to 1880, 122,668 tons was the largest quantity milled in any one year. Whilst Mr. Collins' statement of the tenor of the ore in 1891 is, I believe, unquestionably correct, I venture to think he places the value of the ore per ton rather too low. He values it at 6s. 5d., but, if I am correctly informed, I am inclined to believe it could not have been worth less than 6s. 10d. The selling price of Atlantic copper in 1891 is officially reported to have averaged 12·6 cents. per lb., and if the produce is reckoned at ·65 per cent. on this basis the value of the ore ought to have been $1.638 = 6s. 10d. per ton. The difference in our figures is made up by adding 5d. to Mr. Collins' estimate, which, it

the same as 1891), $1·55, including in this sum smelting and freight, which cost 1·2 cents per lb. of copper smelted, and office expenses, salaries, taxes, &c., reckoned to have cost ·3 cents per lb. of product. Adding the profit of 12 cents per ton milled in 1891 to $1·55 costs, would make the value of the ore $1·67, or a trifle higher than I have reckoned it in 1891. The total cost in 1890, and also in 1891, amounted to 11·8 cents. per lb. of metal, and yielded in the former year a profit of 3·1 cents per lb. of copper, or 40 cents on the ton of ore. Considering the amount of work involved in mining, sorting, and transporting the ore from the mine to the works, stamping and concentrating a rock containing only ·65 per cent. of copper, shipping the concentrates a thousand miles to the smelter, smelting, refining, and marketing the product 1,500 miles from the mines, paying wages, which average $45 to $55 per head per month for the entire force at the mines, and then declaring dividends, it certainly is a remarkable case of skill and economy, even under extremely favourable conditions. Several causes have contributed, I think, to these results, amongst which I would place—1st, large outputs; 2nd, the introduction of rock-drills and high explosives, which has reduced the cost of stoping at the Atlantic from nearly $18·00 = 75s. per fathom (the figure it stood at in 1874) to about $4·33 = 18s. 0½d. in 1891. Drifting per foot has likewise fallen from $14·42 = 60s. 1d., to $4·92 = 20s. 6d. In shaft sinking so much depends on the amount of water met with, as well as the size, section, and position of the shaft, that it has not been possible to show the same decrease in cost, but, with power drills and improved organisation, the cost has fallen from $30 to $36, down to about $18·00. The introduction of drilling machinery was not at once a great economy, but as experience extended and miners on contract earned higher wages by their use, their economy became more and more evident, and affected not only the cost of breaking ground per foot or fathom, but also the rapidity with which the ground can be laid open and mined, hence the amount of dead work has been reduced, and the work can be pushed in every department

with an amount of energy hitherto unknown. It would be absolutely impossible to attain the great outputs of to-day by the old methods of work, no matter how many men were employed. More work per man per day is accomplished through greater facilities provided for doing it, and the high pressure energy infused into every department, by men striving to earn higher wages, by the use of labour-saving appliances; hence the great reduction in the cost of work has also been accompanied by a practical increase in the remuneration of the workman. The general conduct of the work in and about these mines has undergone a progress scarcely less important than that of breaking ground, and the perfection of the appliances for haulage is another and third cause of economy in working such low grade ores. For instance, the Tamarack Company hoists the ore to the surface in double cars through vertical shafts, using compound engines of the Reynolds Corliss type, 42 inch cylinders, and 84 inch stroke, fitted with 30 feet straight-faced drums, winding at a speed of 3,000 feet a minute. The ore, on reaching the surface, is dumped direct into the rock-breaker house, which is connected with the shaft house by shoots, in passing over which the fines are screened into storage bins, whilst the lump ore goes to one of several Blake crushers, which vary in size from 9 in. × 15 in. to 18 in. × 24 in. The bins have a capacity of 1,200 tons, and the crushed ore and fines are discharged from these

a solid iron bed, upon which the anvil and mortar box rest. This change from the old time spring-timber foundations has resulted in an increase of 10 to 12 per cent. in the crushing capacity of the heads, and a decrease in the cost of repairs, which were formerly very heavy, owing to the frequent breakage of the timbers. These stamps are provided with 20-inch steam cylinders, and are run at a speed of 102 to 103 blows per minute, with a drop of 24 inches. The shoes are of remarkably good quality iron, manufactured at Hancock, Michigan. One of them was exhibited at the World's Fair, which had run for 5 days 11 hours, crushing in that time 1,363½ tons of conglomerate to pass a $\frac{3}{16}$-inch mesh This was accomplished with a consumption of 1 lb. of iron only for every ·3906 lbs. of rock crushed. The V-shaped boxes Mr. Collins alludes to are known, I believe, as Richards and Coggin separators. The revolving tables are triple-decked buddles of the Linkenbach type. The concentrates are of two grades, carrying about 23 per cent. and 84 per cent. of metallic copper respectively. These products are sent to the company's smelter at Torch Lake or to the Lake Lindon and Buffalo works. Mr. Collins also mentions that the proportionate loss of copper in dressing at Lake Superior is very small, but on that point I venture to differ from him, as it appears from a statement of Professor Munroe that the loss ranges from 28·5 to 31 per cent. of the mineral in the ore. At the Osceola Mine, to which Mr. Collins alludes, the rock is harder than at the Atlantic, as is shown by the higher cost of stoping and drifting, but I should be inclined to doubt if it is more economically managed than the Atlantic. It is true that the cost of producing ingot copper in 1891 was only 10·1 cents at the Osceola compared with 11·8 cents at the Atlantic. But this was due to the greater relative richness of the ore, which carried 1·4 per cent. of copper in the one compared with ·62 per cent. in the other case. On the other hand, the total cost per ton of ore handled —297,030 tons—was only $1·55 at the Atlantic in 1891 compared with $2·82 at the Osceola, which milled

234,325 tons. The cost at the other Lake Superior mines in 1891 is estimated to have been as follows:—

	Tons crushed.	Per lb. of Ingot. Cents.	Per ton of Ore $ Cents.
Alouez ...	96,164	18·0	2 35
Quincy ...	263,678	9·1	3 69
Tamarack ...	282,987	6·6	3 32
Calamut ...	5,432,927	10·0	6 62

The dividends paid by the Quincy in 1891 amounted to 40 per cent. on the capital of the company, whilst the Calamut and Hecla paid a larger dividend than any other company in the United States—viz., $2,000,000, bringing the aggregate of its payments up to the enormous total of $36,850,000. The dividends of the seven chief Lake Copper Companies amounted in 1891 to about 85 per cent. upon the average market value of their shares. In 1890 the rate was only about 7·5 per cent., speculation having carried the price of the shares higher in proportion than the profits the mines made in 1891, as their gross profits were larger. The average price of Lake copper in New York in 1890 was 15¾ cents, whilst in 1891 it was 12·9 cents. It would seem from these figures that American investors expect to realise 8 to 8½ per cent. on investments in Lake Superior mines. The Osceola

which treats of "mechanical concentration," under Section II. The author remarks that stamping and buddling are only employed in the last resort, because of the serious losses which occur in the slimes thus produced. Now, I endorse his views as to the undesirability, and as to the effect of stamping, but I scarcely think he gives enough prominence to the part which buddling plays in all dressing operations. Stamping, it is true, is seldom resorted to in copper ore concentration, but crushing by the Cornish or other rolls is a very common practice, and no one present needs to be reminded that in crushing copper ore to a size suitable for jigging a large proportion is invariably reduced to a grade too fine for the jigger, and only adapted for buddling; and if the pulverised ore be properly graded prior to concentration, as it should be, a large share of it must perforce go to the buddles, or some kindred machine. Buddling (and under this term I include the work done by the Rittinger Heerde, and other percussion tables, frames, and similar appliances)—buddling, then, in all cases where crushing or pulverising has to be resorted to, is an important factor in the wet concentration of copper ores—often as important as jigging, and is almost universally followed. Now a word or two as to dry mechanical concentration. The author remarks that "dry mechanical processes, though frequently attempted, have not yet proved commercially successful in any place known to the writer." Broadly speaking, my experience agrees with this statement, but there is a kind of dry dressing which, though of somewhat limited application, is sometimes adopted with success, and which has the merit of being simple and economical. I refer to the process known in the Cornish mines as "dousting." (I presume the term has reference to the separation of the dust from pulverised ore.) In wet concentration the principle involved is, of course, the separation of the gangue from the ore by virtue of their different densities, or specific gravities. In dousting, however, quite another property is taken advantage of — viz., that the ore is generally softer, more friable, and falls more readily to powder than

the gangue or veinstone. If you sift the fine powder from a quantity of crushed ore, you will find that this portion is richer in copper than the coarser portion. Only the other day I had occasion to submit some refractory ore to various tests—vanning, jigging, &c. (I mean by "refractory" that the ore was so finely disseminated throughout the matrix, and so mixed with magnetite, that it was extremely difficult to concentrate it). The ore had been pulverised by edge runners to pass a 20 mesh screen, and it carried about 8 per cent. of copper. I sifted the flour or dust from it—that portion which would have gone away as slimes if submitted to wet concentration—and found that nearly 60 per cent. of it would pass a 60 mesh screen, and that this gave an assay of 10·85 per cent. copper; that is to say, about 80 per cent. (81·8 per cent.) of the copper contained was found in the fine powder or dust. This illustrates what I am going to say about the method of dry concentration called "dousting." By this method they used to concentrate and enrich some of the ore in the good old days of copper mining in Cornwall, and I have also seen the process successfully adopted abroad. Let me mention a case at the Cape copper mines, in Namaqualand, the bulk of our ores was sulphides—chalcopyrite and erubescite—and these ores, associated with a hard siliceous gangue, were readily amenable to dousting, which I had the satisfaction of introducing there. We trained our Hottentot, Damaras,

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quality of the ore available, and by the quality required; the bulk of the ore was found in the fine powder, and in this way a large proportion was obtained rich enough to go to the clean ore heap. The coarser part, which consisted mostly of hard gangue, and a little ore, was subsequently crushed finer, and cleaned by jigging or buddling. I cannot quote exact figures, but of the 700 tons of 30 per cent. copper ore we used to raise monthly, about 30 per cent., I should say, was produced by dry concentration. I have followed Mr. Collins' remarks on copper precipitation with much interest, as I have had opportunities of visiting some of the works he has referred to, as, for instance, the Mona Mines, the Pary's Mountain Mines, and the Devon Great Consols Mines. As to Devon Consols, it was the practice there for many years to precipitate the water coming from the mines; I am not aware if this is still being done, but I believe the copper contents of the water has fallen off very considerably for several years past. Referring to the year 1886—87, the water from the old Wheal Maria Mine contained, I am informed on good authority, an average of 10 grains of copper per gallon, of which about 8 grains, or 80 per cent., were recovered by precipitation. Wheal Josiah Mine water contained about 6 grains to the gallon, of which about 4 grains, or, say, 60 per cent. were extracted. These figures correspond very nearly with those given by Mr. Collins. At the period referred to, the quantities of precipitates sold for the year were: from Wheal Maria, 12 tons of 50 per cent. copper; Wheal Josiah, 15 tons of 50 per cent. copper, and 2 tons of 20 per cent. copper. Wrought iron was always preferred as the precipitant, although cast iron was also used. The consumption of iron was rather more than 4 tons to 1 ton of pure copper contained in the precipitate. The stronger the solution, the richer the precipitate, as Mr. Collins points out. It was found that it was not remunerative to treat water containing less than 2 grains of copper per gallon. Since 1886—87 the mine water has gradually become more sterile, and the quantity and quality of the precipitates have greatly fallen off. In 1890—91 only about 2 tons

precipitate was obtained, containing 17 per cent. only of copper.

Mr. Thomas Rickard: I have only within the last two hours received a copy of this paper, so that I am afraid I am rather unprepared, but the range embraced is so wide, and there are so many subjects to talk about, that I shall be able, I suppose, to make a few remarks on certain points that may not be out of place. Referring to Mr. Garland's remarks as to buddling, there can be no doubt in the mind of anyone who is practical on the subject that the section of the mechanical treatment of minerals which is most defective is that of buddling, and I think we have to desire and devoutly wish for improvement particularly in that department of dressing. So far as I know, there is hardly a dressing establishment in existence which does justice to slimes. There is another point with regard to this which is important—namely, that the slimes, not being separated at the very beginning, always filter through the sands, and the sands, therefore, take away a very essential part of the value of the slimes. There is little doubt that the proper method of dealing with slimes would be to separate them at the very starting point after the crushing. I think Mr. Garland was himself once connected with an operation in mines in Switzerland where that method was adopted most successfully, and I am astonished that it is not being introduced more in

a big order to apply "dousting" or winnowing to a mine producing a million tons annually. It is conceivable, but it seems hardly to come within the domain of the practical. It may be a very good thing in the Cape—in Namaqualand—no doubt it was in the particular circumstances described; but I very much doubt whether in handling such stuff as they have to deal with at the Rio Tinto and other mines, that process would be adaptable. Although the subject seems to be old, and, to some extent, familiar to most of us, yet it has within its range several new things. One particularly is that of the adoption of the Manhés process, and I hope Mr. Collins, who dismisses this part of the subject with a few modest lines, will amplify it a little more in his reply, and give to it the prominence which it merits. I saw the Manhés process at work during a visit I made now four or five years ago, and I thought at the time it would lead to very important developments. I eagerly scanned the paper when I received it, hoping that I might find the working of this process described in detail with its results to date. The author has dealt very fully and very ably with the great question of smoke—the smoke nuisance, as it is called, and a nuisance, indeed, it is; the question of smoke in London being, perhaps, hardly so serious a subject to us as this is to the good people down at the various mines in the Huelva district. The question has for a long time been a subject of the gravest consideration to the authorities and exploitants alike, and there has been a good deal of effort to devise some means to overcome or mitigate it. It would seem from Mr. Collins' paper that the Doetsch process shows something like a probability of an approximate solution. The author dismisses rather more curtly than one could have desired the question of the effluent waters. One would have supposed that with so many first-class scientific men as have been connected with Rio Tinto, there might have been some adequate attempt to utilise in some way or other the enormous quantities of metals carried off by these effluent waters. It seems, however, that one must conclude, as nothing has been done, that they have no economic

value. I am not in a position to suggest what might be the best means of utilising those waters; but the quantity being so enormous, and there being such a constant supply of them in a country so near the sea, one would have supposed that chemists might possibly have suggested some means of utilising them. The process of dealing with the ores at Rio Tinto is not so effectual in saving the copper as would appear on the surface. Certainly all dressing is attended by a great waste of metal—greater than engineers are generally willing to admit; and I think it is evident from the figures given, and from the context of the paper, that a very large quantity of copper is lost. I do not mean to say that that is any reflection upon the work, because we all know that the process by which those ores are dealt with, is practically as near perfection as it can be; yet, in the case of both Maidenpec and Rio Tinto, only about three-fifths of the copper is extracted from the open air calcined slabs by the first lixiviation, and an unknown but relatively feeble quantity afterwards. Now, one would like to know exactly what is the absolute loss. It would seem that that is a point which is capable of being stated, and if it could be stated (if for no other reason than a scientific one) it would be very useful. Then I revert to the question of waste from floods. It is evident that not only does a very large quantity of the copper go away in the waste matter that is ejected by

ways in which this form of electro-metallurgy may by-and-bye become much more important and more largely used than it is at the present time. "The solutions may either be obtained (1) direct from the ores, or (2) from slabs of matte, or (3) of black copper produced by previous smelting." Now, I would submit to Mr. Collins, who, no doubt, is acquainted with the history of electro-metallurgy as applied to copper, that for a very long time black copper has been submitted to that process, and I believe it is in use very largely even now among the refiners in Swansea, and I believe I was the person to propose its introduction there first. It has been in use at Mansfield nearly twenty years, and then again it has been used by Messrs. Siemens in their copper works in the Caucasus for a very long time. That is precipitation from solutions of black copper, but there has never yet, so far as I know, been any operation approaching to success with anything but black copper. I would suggest that there is nothing to indicate, at least so far as is published, that the process applies to the treatment of copper direct from the ores or to mattes. I do not, therefore, think the delightful prospect that the paper holds out to us, of the treatment of copper ores by electro-deposition is a thing so likely to have a speedy realisation. I daresay it is within the recollection of many of the members of the Institution that a very large establishment was put up somewhere between Genoa and Chiavari on the coast of Italy for this particular treatment, and an enormous capital was expended by Genoese under the management of Engineer Marchese. For a time they thought they had found the means of making an entirely fresh start in the metallurgy of copper, but the whole thing failed after a thorough trial and a very large expenditure of capital. Other trials of a similar kind were made here by certain parties who had patents in London. I witnessed some of the experiments, and it was quite clear that they failed through the difficulty of getting a cheap solvent for the matte. There are a number of other subjects that I might speak upon, but I think, Mr. President,

if you will allow me, I will conclude with these remarks.

Mr. GARLAND: It must not be understood, with reference to a remark that fell from Mr. Rickard, that I suggest that dousting is universally applicable, or is adapted for all kinds of copper ores, or even for all the ores of any given mine, least of all for cupreous iron pyrites. Mr. Rickard made some remarks about a mine yielding millions of tons. He had in mind Rio Tinto. It is only adapted for limited application, and for certain kinds of ores, as heavy sulphides, where there is a hard gangue.

The PRESIDENT: I think, Mr. Garland, everybody understood what you meant to convey, and the bulk of us understand that that process was more especially adapted for rich ores such as you had out at the Cape Copper Mine, where, having very little water, you wanted to make as much as you could of the air. As regards Mr. Charleton's remarks about the great improvement in the Atlantic Mine at Lake Superior, I think the progressive improvement was in a great measure due to the improved machinery which they employed there between the periods to which Mr. Collins referred. If I remember rightly, in its earlier days the Atlantic Mine had a very hard struggle for existence, and although the percentage of copper was, I think, lower than in any of the mines on Lake Superior, still it became one of the greatest successes. But they had to reform the whole of their dressing machinery, and they paid very great

croust. Going to have a "croust" originally came from having a crust, and so the derivations are approximately parallel at any rate. After all, "dousting" seems to be very much on the principle of some of these machines which have been exhibited in the City within the last three or four years for blowing the dust away from the gold. I do not know whether they ever met with any extended practical application. Unless any other gentleman has any remarks to make, I will call upon Mr. Collins to review the criticisms which have been made upon his very admirable paper.

Mr. Collins: Mr. President—To the remarks made on the last occasion I replied then. Mr. Charleton has not given me much ground for reply. He has given us a great many additional figures in connexion with the Atlantic Mine which I think will be of service, but most of them deal with mining operations which were outside my subject. The same may be said of dividends, and of methods of handling which are common to every kind of ore. I have tried, knowing how wide the subject was, to limit my remarks to those special points which came within it. Mr. Garland's remarks I listened to with a good deal of interest. He was referring to the dusting or "dousting" out of fine particles of rich mineral from high-grade ore. We have nothing to do with high-grade ore just at present, and I think he will admit that the slimes and the dust from low-grade ores are only in exceptional cases worth saving at all. The most extreme case I ever heard of is the one I refer to in the paper—where extremely poor cupreous pyrites were treated in Röraas, in Norway, with a certain amount of success. Mr. Rickard's remarks were, I think, very interesting on many points. I will say nothing more about the slimes. He and I are pretty clearly of one opinion there. With regard to the very interesting Manhés process, it is quite enough for a paper by itself. Like the Hollway process, it is really, as we all know, one form of development from Sir Henry Bessemer's discovery. When sulphur is combined with the metal, you can utilise that to get the metal nearer to purity; and so, whether we speak of the Hollway

process or the Manhés process, or give the process the different names it has lately received in the United States, it is essentially the same thing—burning out the sulphur contained in the ore, and using the heat so produced to bring the material a stage further forward. It is a very interesting subject, but it would have taken me too far from the limits I laid down for myself, and would have made the paper too long if I had said more about it. The reason why smoke is produced in such large quantities at Rio Tinto more particularly, is that it is cheaper to waste the sulphur than to save it. There is no other reason than that, and the inconvenience has to be put up with. Some day, if it gets too troublesome, I suppose it will be put down; but, in the meantime, it is purely a question of cost. And so as to the bye-products of the effluent waters at Rio Tinto. It is possible that I may trouble the Institution some day with some remarks on this question of bye-products, the consideration of which was also excluded from the aim I had before me. There are three in particular—iron sulphate, zinc sulphate, and ochre —which exist largely in the effluent waters at Rio Tinto. Many experiments have been made upon the subject, and it has been clearly ascertained that, under the existing conditions of the market for such products, it does not pay to get them out. As to the loss from not getting more than three-fifths of the copper from the burnt ore at the first

compared with the quantity which is obtained. It is a small relative loss, and the reason why it has been so much in the past is that the works at the mines have been continually growing; the product of the mines has been continually out-stripping the arrangements for treating it. But when you remember than there are reservoirs at Rio Tinto containing, I think, 10,000,000 cubic metres of water, you will see that storage has not been lost sight of. With regard to electro-deposition methods, I did not know that I had spoken very hopefully. Perhaps I did not express myself quite clearly. I did not suppose that we should find an electrolytic method of getting copper direct from the ore. What I meant was getting it direct from the copper solution, and I am not sure that there is not some hope in that direction. Metallic copper has been obtained electrolytically from copper matte—I cannot say with much success from an economical point of view, but certainly in quantities of many tons at a time—and it is just possible that some improvement may be made in that process. With regard to its extraction from black copper, as Mr. Rickard says, that is a well-known process, and a very successful one. The few remarks with which the President favoured us need no reply. It is, as he said, that although the Alderley Edge ores were extremely poor—probably the poorest oxidised ores ever treated on a large scale, yet they could be and were treated for a time with profit, because of the high price of copper, and the fact that the acid was got practically for nothing. I do not know that I have anything more to say, gentlemen, except to thank you all for the kind manner in which you have received the paper and my following remarks.

Mr. Vautin then read the following paper :—

THE "DIRECT" METHOD OF PRODUCING REFINED COPPER.

By Claude Vautin (*Member*).

This method (patented Nov. 21st 1890, No. 18,898, by T. D. Nicholls, Christopher James, and the Cape Copper Co.) is based upon the well-known reaction between oxide of copper and sulphide of copper upon their being melted together, sulphurous anhydride being given off, and metallic copper remaining, which is directly refined in the furnace in which it is produced. White metal is the product preferred for the operation, and this is crushed through a 3-8ths screen, part of it is calcined nearly sweet, mixed with the requisite proportion of uncalcined and filled into a refinery furnace, melted, skimmed, refined, and laded into ingots cake, &c., as may be required, the operation of melting and refining taking the same time as if a charge of pimple or blister copper were operated on. This method has been working continuously at the Cape Copper Co's.

puts through about 10 tons a day running continuously and calcining the metal much better than the old calciners. The calcined metal is sampled, and the sample mixed with a portion of raw metal—melted in a crucible, and, from the result of this trial, the proportions of the refinery charge are determined; if the button of copper is covered with slag, then the proportion of raw metal must be increased, or, if the button is coarse or covered with regale, it requires more calcining, the pitch aimed at being just below blister copper when the whole charge is melted. The weighed proportions of raw and calcined metal are now mixed for a 15 ton charge, say, 9 tons of revolver calcined and 6 tons raw, or 11 tons reverberatory calcined metal and 4 tons raw metal. The mixed charge of raw and calcined metal is filled into the refinery whilst it is still hot from the previous ladleing, the furnace closed up and fired hard. In 4 or 5 hours the charge softens and flattens down, copper forming on the surface, and, as this hot copper penetrates the charge, a gentle yeast-like working takes place, dense fumes of SO_2 are given off, so much so that the furnace flue is often inadequate to carry them off quickly enough, and they force out through every crack, the heat produced by the chemical reaction aiding the reduction, and the whole charge is quickly melted down. The proper mixing of the charge having been carefully attended to, very little slag is made. The furnace is now skimmed, when it takes about half an-hour's rabbling to set it, and it is ready for poling as if ordinary blister had been used. Each refinery works a charge of 15 tons metal, making 11 tons copper every 24 hours.

Various trials were made at the commencement of working this method to ascertain the best (and, of course, the cheapest) pitch to which the metal might be calcined in order that the refinery work should be properly done, and it was found that although when the metal was calcined to black oxide it would decompose its own weight of raw metal, and when calcined to red oxide it would only decompose about half its weight of raw metal, yet

the cost of the extra calcination was so heavy as to counteract the saving by using more raw (and consequently less expensive) material. In practice it is found that the calcined metal often contains from 1 to 1½ per cent. of sulphur, and yet requires the same weight of raw metal to properly decompose it, as if it were all red oxide; this being probably due to the formation of portions of sulphate of copper during the calcination, the oxygen of which reacts on the sulphur of the raw metal when melting in the refinery furnace.

The introduction of this method of working at the Cape Company's works has proved in every way a success, and no money was expended in experiments. The first charge tried proving as great an improvement as each succeeding one; the only change made at the works being the flowing out of a large number of roasters and turning into cash the copper locked up in their bottoms. The fact that about 14,000 (fourteen thousand) tons of ingots of excellent quality (*best select*) have been made this way, and that not one ingot has been complained of, whilst the ingots made from blister copper out of precisely the same materials have now and again failed to give complete satisfaction, speak highly in favour of the "Direct" method. A careful comparison of the old roasting process for the twelve months preceding the introduction of the new, with the first eight months'

per cent. *less* slag to be re-treated, whilst the actual cost per ton of ingots made was 13s. 6d. per ton in favour of the Direct process. Now, it costs just as much to make white metal, whether it be afterwards turned into copper by roasting, or by the "Direct" method; but the proportion of that cost to be borne by each ton of ingots is very different, as will be seen by the following: If it costs £4 per ton to make white metal, it costs £400 to make 100 tons white metal; if this white metal produces 54 tons of ingots by being roasted, the cost of making white metal will be £7 8s. 2d. per ton of copper; or, if it be treated by the "Direct" method, and produces 74 tons copper, the cost per ton of ingots be only £5 8s. 2d. per ton of ingots, a saving of £2 per ton of ingots in the making of white metal, in addition to the saving of 13s. 6d. per ton of ingots in the actual treatment of the white metal after it is made.

The advantages of the "Direct" method over the roasting method may be summed up as follows :—

A much larger yield of copper ingots per ton of white metal made, thus increasing the output of any present plant.

An excellent and uniform quality of copper.

Great reduction of cost.

No expense in starting the method, since all copper works have crushers and calciners.

No especially skilled labour wanted, the ordinary furnace and calciner men working one process as readily as the other.

In starting a new works on this method less capital is required, because no copper is locked up in roaster bottoms.

An old works adopting this process can turn the immense quantity of copper in their roaster bottoms into cash.

Crucible experiments show that the many impurities found in copper ores can readily be got rid of by making more slag, arsenic and other volatile impurities being carried off completely, whilst those that must be slagged off by roasting can, by the "Direct" method, be cleaned up in about 1-3rd of the quantity of slag produced by roasting.

DISCUSSION.

Mr. J. H. Collins: I am very sorry that there is not some copper smelter present to deal with Mr. Vautin's very interesting little paper. To me the new process seems extremely simple, and I wonder why it was not found out before; but I cannot help a sort of suspicion that there must be a little more to be said against its general use than appears just at present. For instance, notwithstanding Mr. Vautin's concluding remarks I should expect that this method would not be very readily applicable to copper obtained from very impure ores, though it may be very suitable for such ores as those of the Cape Copper Company. With regard to rolls, the Krom rolls differ from Cornish rolls in two respects. One is that they are driven generally by

Colorado, and is recognised as one of the leading metallurgists of the United States. Now, Mr. Pearce has gradually developed what seems to me to be the most perfect roasting furnace ever yet built. I can give you very easily a general idea of it. It is an annular furnace. The hearth forms a complete circle except for a small arc of 4 or 5 feet, and the ore is fed in continuously at one side of the gap. It is carried round at a rate which can be varied in a very simple way by rabbles which discharge at the other side of the gap. You can regulate the furnace working by the amount of material you feed in, and by the pitch of the rabbles which discharge the roasted material. These rabbles are continually stirring it up. The fire playing through the chamber is aided by a very gentle blast, which is just sufficient to keep up a continual circulation of fresh oxidising air. A furnace capable of treating 26 tons per day is easily worked by one man in each shift who attends to everything, so that with dear labour, and not very good coal, the total cost of perfect roasting—as perfect as I have ever seen it—does not exceed 50 cents. a ton. I think it will be very hard to beat that.

Mr. D. A. Louis: I agree with Mr. Collins that this seems so extremely simple that it is surprising it has not been introduced before. With the small though long acquaintance I have with the Swansea process, it really pains me to think that, according to Mr. Vautin, it is apparently doomed to disappear. It always strikes one as a monument of industry and perseverance; for the development of the Swansea process to its present state must have been the result of years of careful thought and practice by various generations of metallurgists who have gone before us. I think Mr. Vautin has overlooked something in that roasting furnace; it is extremely interesting to watch this process going on. The white metal is put in in lumps, and is heated comparatively slightly to start with; as it gradually melts, the material trickles down, and not only does much oxidation at the surface take place with the production of a certain amount of copper and its sub-oxide; but inasmuch as this oxide dissolves in the copper and mixes in the

molten mass, it, at the expense of its own oxygen, effects the oxidation of undecomposed matte. After it is melted and ceased to continue at the surface, one can then see an evolution of gas going on all over the mass, which, in fact, gives rise to the so-called blistered appearance. Now, I think the point Mr. Collins touched upon as regards the purity of the ore was very cogent. By the Swansea process of roasting the white metal, there is every opportunity given for various impurities to go away steadily—iron, for instance, small though its proportion may be—into the slag, arsenic and antimony by oxidation and volatilisation. In the process suggested by Mr. Vautin, portions of the white metal, with all its impurities, is going to be put into the refinery, and charged along with roasted matte. In that case it seems probable that there may be greater difficulty in getting rid of the arsenic, &c.; hence, whilst successful with a pure matte, the suggested operation might not prove economical with a very impure matte or ore. With reference to the fact that some of these coppers have proved to be equal to other coppers said to be best selected, it is not much recommendation unless the chemical characters of the latter were ascertained. The point seems to me to be this: the old process does get rid of these impurities in a very thorough manner, and it has always been thought extremely undesirable to put into the refinery any sulphury matter or any impure matter; this, therefore, seems to be a most

necessitate heating for a considerably longer period, and a consequently greater outlay. This, however, is very thoroughly roasted, and volatile impurities would naturally be got rid of. This material which has been obtained at this extra expense and purified by this vigorous roasting, is mixed up with stuff from which impurities have not been removed, and the purification of the latter is left to the chance of its coming in contact with particles of infused oxide. If this contact is good, the purification should be good, and *vice versâ*. I think Mr. Vautin said it required rabbling. One knows that this mechanical means can never be so perfect as the mixture by fusion I have mentioned. It is well known that in the ordinary process the mass becomes in the manner already noticed, impregnated with cupreous oxide, and should be quite as well or even more thoroughly purified than by any mere mechanical mixing with this oxide. I have jotted down a few points that I thought would be interesting to know. They are: what quantity of metallic copper and of what chemical quality is produced in a given time from ore or matte of known composition, and with how much fuel and how much labour; and upon what size of furnace, because the size of the hearth and the fireplace are very important points. I think Mr. Vautin's paper is extremely interesting, and the subject is a very attractive one. It is very gratifying to hear of these practical applications of chemical reactions in metallurgical operations.

Mr. Thomas Rickard: I think Mr. Claude Vautin, who has given us this paper, has quite taken away our breath; at least, he has mine. The most wonderful part of it is that the process seems to come full fledged into existence; and in his exultation the author scarely condescends to give us the genesis of this discovery. Now, I do not think that at Swansea there would be that amount of surprise at this thing that he seems to suppose. After all, the reactions have a very strong family likeness to those which take place in reverberatory lead smelting, in the ordinary Welsh and Silesian furnaces. Now, if that is so, it is no new thing; and, if it is no new thing for us

generally, it certainly should not be for Swansea smelters. With Mr. Collins, I regret deeply that we have not here someone representing the dear old Swansea method that he speaks so irreverently of. [Mr. Vautin: "Not at all."] The Swansea process has existed from the time of the Chaldeans down to the present, and I think we shall hear of it even after this new process has told its story Nevertheless, this paper is one of the most wonderful announcements in the domain of metallurgy that has been made, perhaps almost at all — not even omitting the Newbery-Vautin and the MacArthur-Forrest processes. In replying, I hope Mr. Vautin will favour us with some idea as to how far it is applicable to lower grades of material. I am of the opinion other speakers have given expression to—that it is not applicable to any but very rich material; and, then again, we do not know, we have not yet heard, what difference there is in point of loss. I should suppose the loss will not be larger; but, at any rate, we want to know what difference there is. Then, I think the reference made to calcining in this paper is one of its remarkable features. I was delighted to listen to what Mr. Collins has said about the new calciner contrived by Mr. Richard Pearce, of Denver. But even that is not a new thing. It is the old tin calciner which we have known in Cornwall. It is, in principle, the same thing; at least, so it seems to me. I hope, Mr. President, we

through the super-heated atmosphere produced in the furnace. I have seen the working costs, and I know it works at a very low rate indeed. Whether it is cheaper to operate I cannot say, but I think all these round hearth furnaces are derived from the old English type alluded to, and are some of the best roasting furnaces, especially where you have to treat very fine ore liable to dust. Working as they do, at a slow speed, they are able to roast ore which could not be treated in a revolving cylinder. Do I understand that £7 8s. 2d. is the cost of refining the white metal?

Mr. CLAUDE VAUTIN: If you divide the £400 by the 54 tons of ingot copper produced, that will give you the cost; and if you divide the £400 by the 73·4 tons, on the other hand, you get the difference in the cost.

Mr. CHARLETON: That is the refining cost?

Mr. VAUTIN: Not exactly that. It is the cost of producing the white metal as well.

Mr. CHARLETON: It would be interesting to know if Mr. Vautin can give us some figures as to how this would compare with the Manhés process. Producing copper pig at Butte, containing 99 per cent. of copper, the cost per ton of matte is £1 8s. 9½d., or £2 16s. 5½d. per ton of copper—equal to two-thirds of a cent per lb. of copper produced.

The PRESIDENT: With reference to Mr. Pearce, some of the gentlemen here present incline to the belief that a bit of the furnace rotates. I gather that is not so.

Mr. COLLINS: It is not so. Nothing rotates, except the arms carrying the rabbles. I think those who are interested will find a series of sketches in the *Engineering and Mining Journal* some time in the middle of last year, and if they wait they will find a good description in the *Transactions* of the American Institution of Mining Engineers.

Mr. CLAUDE VAUTIN: I was quite conscious, Mr. President, that I should lay myself open to severe attack, but I hope to be able to repulse the attackers; and I am very pleased to say that two of the gentlemen have given me the weapons whereby to do so. Referring to rolls, it has been

my experience that when you are dealing with a material of the consistency of white or blue metal, if you only want it to pass through a three-eighths mesh, the higher the speed of the rolls the better. In a high speed belted roll—I won't use the word Krom—I find that the amount of slime or dust produced is fully 50 per cent. less than the quantity produced with the slow motion or Cornish rolls. When you have got a nice material, such as copper sulphides, in the condition of white metal, then I would not hesitate to condemn the old Cornish rolls, and give preference to the belted rolls. Mr. Pearce's roaster has been referred to by Mr. Collins. You all know the O'Hara furnace. It is a very excellent furnace for what it has been designed, and Mr. Pearce, who undoubtedly holds one of the first metallurgical positions in the United States, has simply bent the O'Hara round into the form of a horse-shoe. But while we are speaking of calcination, it is just as well to bear in mind the object of the special calcining furnaces. For preparing ore as one of the intermediate processes for copper smelting, the Pearce furnace would be a most excellent one, but I doubt if it is of any use for a dead sweet roast such as is required either for amalgamation or chlorination. When you want to reduce the sulphur to only 5 or 6 per cent. the furnace would be a most excellent thing, and its use will undoubtedly become very general. While I am on the subject of calcining furnaces I would

a very excellent thing for what it has been designed—namely, the oxidation of iron pyrites in the presence of blende and galena, so as to enable the subsequent separation by mechanical means. I think Mr. Collins said that for the pure ores of the Cape Copper Company this refining process may do, but not for impure ores. I am delighted that that remark has fallen from that gentleman's lips. I have with me three small ingots of copper which were produced from some matte sent up to Mr. Nicholls of the Cape Copper Company. The owner of it stated, "If you can make copper from this matte that will stand the brass test I shall be surprised." Good copper was made much to the surprise of those interested. Why, I consider this process is as good for pure as impure ores is that you are able to control the amount of oxide of copper to be mixed with your sulphide; and, should the matte have been produced from impure ores, by having a slight excess of oxide of copper over and above that necessary to reduce the sulphide, and then submitting the mixture of the oxide and sulphide to melting heat in the refinery, slag is produced all through the mass simultaneously (in consequence of the excess of oxide of copper), and thus you are able to remove impurities which would remain in the copper by the ordinary process. Hence it has been possible to obtain much better copper in one operation from impure mattes by the "Direct" method than by the ordinary roaster process. The impure metal referred to contained a considerable quantity of impurities.

Mr. J. H. Collins: What sort of stuff was it?

Mr. Vautin: It contained 2 or 3 per cent. of arsenic.

Mr. J. H. Collins: Any antimony or bismuth?

Mr. Vautin: None.

Mr. Rickard: How much copper?

Mr. Vautin: 70 per cent. Referring to bismuth, the chances are that if it is in a metallic condition and once alloyed with the copper it would be a most difficult thing to eliminate it. Referring to Mr. Louis's remarks, as a rule there is very little iron in the white metal state. The reason iron and arsenic are removed I explained previously—namely, in consequence of the heat being produced all

through the mass, and by the oxidising action of the excess of the oxide of copper, they have been able successfully to deal with mattes containing impurities in the form of arsenic, &c. Roasting down in the way that Mr. Louis referred to in a rough copper roaster, what do we get? We actually find you cannot turn out more than two tons of blistered copper in twenty-four hours in ordinary working, and that requires an enormous quantity of fuel—say five tons—and produces, on an average, about 34 to 35 per cent. of roaster slags. On the one hand, you have got all the wear and tear of the furnaces, all the labour attached to the roasting; whereas, on the other hand, you have this result, which actually occurred. Twenty-two rough roasters were flown out. These 22 roasters treated, say, about 4 tons of white metal. Now, to work that out, 88 tons of white metal could be treated in these 22 furnaces in 24 hours; but by the "Direct" process only two-thirds of that amount would be calcined. Say 54 or 55 tons would have to be roasted in revolving mechanical roasters which, according to books kept in the most careful manner, only consumed 22 cwts. of coal in 24 hours, and put through 9 to 10 tons of crushed matte. On the other hand, there is not a single one of the ordinary roaster furnaces that burns less than 5 tons per 24 hours. The saving in the consumption of coal is as cwts. to tons, to say nothing of the wear and tear. And, again, in

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reason is simply this—that knowing how highly prejudiced copper smelters are, as a rule, the gentlemen interested in this process have actually had it at work for three years, keeping the matter as close as possible before venturing to make it known. In three years they have actually turned out 20,000 tons of copper by this method, and I know of one institution which has saved over 60,000 sovereigns in costs, and taken £25,000 worth of copper from the bottoms of the old roasters which had been flown out. Referring to the dear old Swansea process, which is undoubtedly an excellent one, and to some of those who have the management thereof, I should like to inform you that a gentleman of my acquaintance, having some knowledge of the Siemens' regenerators, attached two to one of his metal furnaces, and he was turning out from the one furnace as much rough metal as from any other four ordinary fired furnace in the works, and had been doing so for many days. He then ventured to call the manager's attention to the fact. That gentleman, after having inspected the furnace with the regenerators attached, and being convinced of the extra output, stated to my friend, "No matter what the improvement is, or may be, it is an experiment, and therefore pull the whole thing down, or you leave these works." And he had to. On one occasion, my friend took two bricks out of the roof of a furnace to admit more air, when the men positively refused to work her till the bricks were replaced. The aperture was closed by a fire clay plug in such a manner that when looking at the roof from the door the plug could not be seen; hence the workmen considered that the holes were still open, and positively swore that they could not make the furnace work, although the apertures had been actually closed for three days! It was not until they were closed in such a manner that they could actually see the brick end from the working door that they could get the furnace to work. Referring to the sorrow expressed by Mr. Louis as to the doom awaiting the Swansea process, that gentleman has misunderstood the purport of some of my remarks in the paper. The "Direct" method of producing refined copper in no way marks the doom of the Swansea process as a whole,

but is only an improvement of a portion of that process, which, as Mr. Louis well knows, consists of many parts. It is simply an improvement in the production of refined copper after white metal has been produced. I am going to be bold enough to say that this improvement in the metallurgy of copper will gradually find its way into every smelting works in the world. Referring to the cost of Bessemerising, I believe it will be much cheaper to calcine, in the way I have described, in revolving furnaces than Bessemerising. Metal blown up to 75 per cent. in converters, and then treated by the direct method, can be converted into ingot copper for less money than the Bessemerising costs to produce rough copper. Under any circumstances, in Bessemerising you have two or three remelts, and then the refining operation. I intend to go very thoroughly into the comparative efficiency of this direct refining method as compared with the Bessemerising of mattes, both as to cost and quality of copper produced, and hope to give the Institution the information at a future date. I may say that I passed a considerable portion of my time in introducing the Bessemerising of copper products into Australia, and therefore can speak with some confidence on the subject.

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THIRD ORDINARY MEETING.

The Third Ordinary Meeting of the Third Session took place at the Museum of Practical Geology, Jermyn Street, London, S.W., on the evening of Wednesday, January 17, 1894, at 8 o'clock.

Mr. George Seymour (President) occupied the chair.

The Secretary having read the minutes of the last ordinary meeting, which were duly confirmed, the Chairman called upon Mr. H. L. Lawrence to read the following paper:—

DRESSING OF ZINC-BLENDE ORES AND MAGNETITE AT THE NEW PIERREFITTE MINES, HAUTES PYRENÉES, FRANCE.

By H. L. Lawrence (*Member*).

The New Pierrefitte Mines contain, among other ores, zinc-blende, intermixed with magnetite, which occurs minutely disseminated through the blende and accessory galena, and gangue. The intermixture is very irregular. Magnetite aggregates will vary from minute quantities to such a proportion as to form a practically massive mineral. The formation also contains small quantities of magneto-pyrite. Very ferruginous varieties of zinc-blende as christophite and marmatite are also present.

On the average this ore may be taken to contain 20 per cent. of zinc-blende, 20 per cent. of magnetite, and 1 to 2 per cent. of argentiferous galena, the remainder

The chief product of these mines is argentiferous galena, which is contained in a granulite gangue, irregularly impregnated with magnetite.

The blende is won from branches and spurs of the main bodies. When this mineral is massive, magnetite is rarely and but sparsely intermixed with it, but is confined to the gangue, so that mining and dressing is of the most ordinary and easy description. Marketable blendo can be procured by taking care to place the shots so as to break a minimum of the gangue; then the roughs are handpicked, yielding pure blende and mixed blende; the latter is stacked, the former passes on to the market.

The mine fines contain only small quantities of ferruginous gangue, as this substance is very hard, and shows no tendency to crumble; a marketable blende can therefore readily be won on jig and buddle, leaving but a small quantity of middlings which contain an excess of magnetite. Not that the marketable blende is quite free from magnetite, but the high metallic contents of the former, and the small quantity of the latter, render an average of over 45 per cent. of metallic zinc possible. When, however, the blende is less massive, then there is invariably more magnetite present, both in gangue and ore. The blende itself is also more ferruginous in composition, and consequently contains less metallic zinc, and the above operations become no longer possible. It was to treat this latter class of ore, and also the middlings and bye-products from the former operations, that the works hereinafter described were erected.

The magnetite can be eliminated before dressing or after. On first thoughts the former would seem the most natural—*i.e.*, to crush down the ore to its required size, pass it over an approved magnetic separator—taking out all magnetic parts—and then proceed to dress in the ordinary way. Following this scheme three products would be made before dressing:—

(1) Ore free from magnetite;

(2) Middlings consisting of grains composed of a mixture of galena, blende and magnetite;

(3) Pure magnetite.

The second product would then be further disintegrated and passed over a second separator and dressed without further difficulty. This method, however, has the following disadvantages :—

The magnetic separator must be reliable, both magnetically and mechanically. It would have to work continuously to keep pace with the output; any slight defect in the electric current or leakage would necessitate stoppage of the whole works. If any such defect were not immediately noticed (and such defect is not easily detected), the whole pulp would be charged with magnetite, rendering it necessary to clean out every jig and buddle and contrivance on the floors. It would have to stand proof against a varying quality of mineral fed to it—*i.e.*, for some minutes it might have masses of magnetite to deal with; at other times there might be little present in the ores. A precise adjustment to minimise middlings under such conditions would not be easy. It would have to be adapted to treat unsized material, which for precise work such as would be required here would be a severe test.

Very perfect magnetical separation would be necessary—*e.g.* : We are working on an ore containing, say, 20 per cent. magnetite, 20 per cent. blende, and 1 to 2 per cent. galena, the remainder being composed of gangue. Supposing the separator to leave 3 per cent. of magnetite with the headings, this will be found after dressing with the

required size, classify, treat the coarser classes on jigs, and the slimes on buddles or tables. The jigs can be made to yield:—

(1.) A product of galena and magnetite, which presents no difficulty in finally cleansing.

(2.) A product of magnetite and blende, which can be magnetically separated, giving—

(*a.*) Pure blende for market.

(*b.*) Magnetite.

(*c.*) Middlings consisting of particles of mixed blende and magnetite; also all magneto-pyrite which is not magnetic enough to pass into the magnetite. The buddles would yield in the first washing:—

(*d.*) Headings consisting of galena, blende, and magnetite.

(*e.*) Tailings consisting of waste.

The headings (*d*) can now be magnetically separated and the final freeing of galena from blende can be effected on Rittinger tables or other appliances.

Comparing the two methods—*i.e.*, dressing before magnetic treatment and magnetic treatment before dressing—I am of opinion the former has the advantage, as the valuable galena is freed from the blende at the outset, and no re-crushing of these two minerals together is necessary, thus avoiding the tedious and often wasteful operation of treating their slimes. Further, the middle product is minimised in quantity and confined to the very poor, and, I might say, almost unprofitable ore, consisting of highly ferruginous blende and magneto-pyrite. The magnetic separator having less bulk to treat can be regulated and adjusted to requirements of each class of ore, which can be passed in rotation as demanded.

Acting on this principle the works hereunder described were erected.

Referring to the accompanying plan and section (Diagram I.), it will be seen that the ore is dumped on to platform (A), and fed to a pair of Cornish rolls (B), fitted with Raff wheel (C). The main sizing screen (D) is wire-wove, and has four holes to the inch. A bucket elevator

(E) carries the pulp to the three classifying trommels (F). The first of these is of Cornish gauge, No. 17; the second, No. 28; the third, No. 36. A Spitz-gerinne I. divides out a fourth class. The four four-hutched Green's jiggers (K, L, M, N) are arranged to receive and treat the classified pulp. The overflowing slimes pass on to two Spitzkasten (O and P), and are separately treated on two Rittinger revolving continuous buddles (Q and R). The waste is here extracted, and the headings are magnetically treated on separator (S); the final separation of galena and blende takes place on two Rittinger percussion tables (T and U). The foregoing contrivances are too well known to need further comment. I will now pass on to the results.

1. *The Jiggers.*—In the first hutches of all four jigs all pure galena, and all galena mixed with magnetite is caught. The second hutches deliver a mixed product of galena blende and magnetite. This is re-jigged on the auxiliary machines (V and X), which are of exactly similar pattern to the others. Here the last remnants of galena are freed from the remaining pulp, which consists of blende and magnetite. Nos. 3 and 4 hutches give blende and magnetite with most of the magneto-pyrite. A certain proportion of this latter mineral can be made to pass over with the tails.

The first or galena products, both from main and

addressed myself to the *Silber & Bleibergwerk Friedrichssegen bei Oberlahnstein,* which company, I understand, have had a magnetic separator, designed and patented by themselves, in use for many years under somewhat similar circumstances. They referred me to Messrs. Sauter Harlé & Co., of Paris, who, they informed me, were their agents in France, at the same time kindly sending me a diagram of their machine (Diagram II.). Addressing myself to Messrs. Sauter Harlé, I found that they did not make an exactly similar machine to that of Friedrichssegen (see Diagram III.). By comparison of the two, it will be at once observed that while both have stationary magnets, the encasing and revolving drum of the former carries the magnetite over it, while in the latter it is thrown underneath; the magnets in the first case being placed in a higher segment of the drum than in that of the second machine.

I thought the Sauter Harlé modification an improvement as it allowed of making a middle product, so necessary to the success of our operations, by partitioning off at point (A). It was decided to order one as an experiment, the price being but 1,500 francs. Electric power needed for the magnets was gauged at 910 watts, which we could supply with the dynamo used for electric lighting.

Experiments showed that the electro-magnetic force was hardly sufficient to quickly and firmly catch up the larger grains, and throw them into the magnetite compartment. Otherwise, by adding a catch-board (B) and the compartment (A) for middlings, the machine was satisfactory. It was therefore decided to arrange for a machine with magnets sufficiently strong to develope a magnetic force corresponding to 1,680 watts, and also to build the framework and bearings somewhat more substantially. Diagram V. represents the same.

The catch-board (A) and division (B) marked in red were added at the mines, after careful experiment as to their position and size. This machine has a capacity of passing from 20 to 30 tons of mineral in 12 hours. The feed has to be regulated according to size of the ore, nicety of work

required and proportion of magnetite to other ingredients. It has been at work here for over a year and has given every satisfaction. The price of same is 4,200 francs f.o.o. Paris. This separator works on dry ores only. The wet products from the jigs are taken on to sloping platforms outside the building to allow the excess of moisture to drain off. From there they pass on to drying floors. (See Diagram IV.) These consist of a double series of brick flues; each series has an ordinary brick furnace. The flues are covered by cast-iron plates 15 millimetres thick, resting on the brickwork. At each cross joint of the plates a rail is thrown across in order to prevent leakage between two plates. The chimneys are 8 metres high. The draft has been found to be all that is required.

The ore once dry is passed on to the separators (S) and (Y), the former being the one first experimentally ordered (Diagram III.), and is sufficiently powerful for treating the fines. The number of revolutions of the drum best adapted for good work have been found to be 56 per minute. The feed can be regulated by opening or shutting the hopper door at (C). The coarser stuff requires slower feed. It is the rule to pass the slimes on machine (S); the product of the coarsest jig is passed separately on machine (Y), while the products of the three finer jigs can be passed jointly on the same. The dry ore is allowed to accumulate until there are 20 to 30 tons before it is treated. One or two days' work with the separators suffices to deal with the week's output.

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	Kilos.
(*b*) Quantity of middlings obtained (Sample No. 5)	18,100
(*c*) Quantity of magnetite obtained (Sample No. 6)	43,500

The middlings have been found to be of such little value, that they have been left on one side pending improvement of the metal market.

In drying the fines the room on the drying floors is found to be somewhat cramped; the dust resulting from this operation also is injurious to the jigged products. To properly handle this material, a separate floor should be erected to dry it; or, better still, a wet magnetic separator adopted. I should fancy the one designed and described by Mr. Walter Basset before this Institution on June 15, 1892, would be a very suitable apparatus.

The cost of dressing as just described, including coal and repairs, is 6 francs per crude ton of ore.

An analysis, made by Dr. Morgan, of Swansea, in August 1892, of the magnetite product is as under:—

Insoluble matter	9·90	per cent.
Oxide of Iron	76·51	„
Alumina	1·29	„
Sulphur	0·40	„
Carbonate of Lime	1·85	„
Carbonate of Magnesia	7·20	„

This ore has not yet found a market on account of its excess of sulphur; an inexpensive roasting would render it an excellent iron ore, for which a ready sale can be had.

There is no doubt that this iron ore will be found to be a great resource to these and neighbouring mines, when it is utilised as a bye product, and no longer regarded as merely an encumbrance to the winning of the more precious argentiferous galena and zinc-blende.

DISCUSSION.

Mr. A. G. Charleton, who was unable to be present, sent the following remarks, which were read by the Chairman :—

Mr. Lawrence alludes to the difficulty of obtaining information about American machines. One of the best of these is, I believe, the Chase magnetic separator. The two chief features of this machine are, first, the smallness of the magnetic wheels, and the peculiar structure of the magnets ; and, second, the fact that it can be worked wet. This latter advantage is very great, for not only does the water lay the dust and wash the mineral, but it also decreases the weight to be borne ou the lower side of the belt. The machine separates the material into heads, middlings, and tails. The ore is fed from a hopper on to a horizontal belt, which moves to the left round the first magnetic drum. As the material passes downwards, the tailings fall into a tank, and when the material comes opposite a row of fixed magnets above the belt, the middlings fall into the next division. The pure magnetite is carried round a second magnetic wheel, and, as it ascends, it leaves this belt, and is attracted by a third magnetic wheel to a belt passing round it. The ore is carried up this belt, and is delivered over a pulley into a receptacle outside the tanks. As the material passes round the second magnetic wheel, and is attracted to the third wheel, any clinging dust and non-magnetic particles are separated from the magnetite. There is a driving pulley and tightener to give motion to the first belt, and to keep it in place.

[redacted]

is made of cotton duck, and it stands the blows of the particles of ore by yielding under it. The chief wearing parts are the bearings of the shaft and belt. The cost of dressing at Pierrefitte on an output of 21,449 tons of crude ore in 1891 was 4·71 frs. = 3s. 10d. per ton. I take it, therefore, that the difference between this sum and Mr. Lawrence's figure represents the cost of magnetic separation, chargeable in addition in 1892.

Mr. JOSEPH GARLAND: This paper has a special interest for me, because it is just ten years ago that I examined the mines for the old Pierrefitte Company. At that time I find from my note-book, that the lodes were very hard, yielding a rather inferior quality of lead ore, not often occurring in pure lumps, but mostly disseminated through the veinstone, and much mixed with blende and magnetic iron ore. The lodes were very expensive to work, and the ores difficult to separate and dress up to the ordinary standard. To quote another passage from my notes of ten years ago, the water-dressed blende, when free from lead ore and gangue, was so mixed with magnetic iron ore as to contain scarcely 40 per cent. of zinc. At that period magnetic separation was not so much before the mining public as it now is, and no trials had then been made with this process at these mines. I have listened, therefore, with great interest to the author's description of the method which he, it seems, successfully adopted to separate the deleterious magnetite from the blende and galena. No mention is made in the paper, I believe, of the quantity of silver contained in the galena, although the author does mention argentiferous galena once or twice.

The PRESIDENT: The proportion of galena is only 1 to 2 per cent., is it not?

Mr. LAWRENCE: Yes.

Mr. GARLAND: I should be glad to know this, as I read in my old notes that the loss of silver in dressing by water was found to be very considerable. I should like to hear what the author's experience has been in this respect at Pierrefitte. It is not the first time I have come across mines where there has been a loss of silver in dressing by water. My friend, Mr. Rickard, can back me up when I

say that that was our experience at a continental mine. The author, in the course of his paper, says, "The chief product of these mines is argentiferous galena, which is contained in a granulite gangue, irregularly impregnated with magnetite." He says this after stating that the galena and gangue are accessory to the blende, signifying thereby, I take it, that they are of less importance. I wish to ask in what sense the galena is the chief product. I presume because of its silver contents. It cannot be because of the lead, for the author points out that whereas there is 20 per cent. of blende, there is only 1 or 2 per cent. of galena. Then I do not quite follow the author's meaning when he says, "Following this scheme three products would be made before dressing—(1) Ore free from magnetite." What ore? Galena or blende? I presume galena. I would like also to ask the nature of the gangue.

Mr. Rickard said he would have to plead guilty to being one of those who were responsible for having allowed silver to escape in the treatment of galena. He, however, did not think it by any means a uniform experience, as he could cite opposite examples which had come under his personal observation. The Pontgibaud mine was an example of the ordinary cases where silver escapes, and becomes greater in proportion as the ore becomes more and more finely pulverised, that is, the

operations did not seem excessive, though with the more automatic machinery which is used now he thought the price might be reduced perhaps 20 or 30 per cent. The success at Pierrefitte added another to perhaps some seven or eight cases of success with that particular apparatus. A remarkable one was that of separating the spathic iron and blende in the Isle of Man. Then there were numerous cases in America where they crushed down their ore and separated the gangue by the same process. He would like to ask the author whether he had ever found this kind of operation adaptable to any other ores than those of magnetite and spathic iron ore, and whether, in undertaking this matter, he did not revert possibly to another means of dealing with the stuff—viz., that of calcining to get rid of the zinc, and reducing the residuum by means of fire. He would not say that it was a preferable process, because the quantity of the blende got out and the price of getting it was so unsatisfactory. He thought, however, it had been used, and that Mr. A. L. Collins, in a paper read before the Society of Engineers, had referred to the treatment of very difficult and complex ores, containing blende, by means of a preliminary calcining, by which they got rid of the zinc. In conclusion, he would ask the author to what extent the tenor of his product was raised by the modification in the machine shown on Diagram V. Mr. Lawrence had given the quantity of marketable blende as 58,400 kilos, obtained out of 120,000 kilos—rather more than 50 per cent. of blende. This ore, he had stated, after concentration was taken to the magnetic separator, and only yielded about 20 per cent. He hoped Mr. Lawrence would explain these points.

Mr. A. L. Collins was afraid he could not add much to the information of the members of the Institution, as he had had no direct experience of magnetic separation on any working scale. He had once made a few experiments on the separation of low grade blende and magnetic pyrites, both by roasting, so as to obtain a soluble sulphate of zinc, and by direct magnetic separation; but they did not come to any conclusion which he could regard as satisfactory. As to one remark made by Mr. Rickard, he might perhaps

be able to give an answer. He asked whether any other ore—that is, other than magnetite or chalybite—had been treated by this magnetic process. Some four years ago, a case in point came under his (the speaker's) notice at Reocin, in the province of Santander, Spain; they had large amounts of ore which consisted mainly of ferruginous calamine, and earthy or calcareous brown hematite, which it was impossible to separate by hand picking or dressing. This ore was crushed and roasted in reverberatory furnaces with an admixture of coal, by which means the hematite was reduced to magnetic oxide of iron. On subsequent treatment by magnetic separators, not unlike those described in the paper, it was found that this artificial magnetite was easily separated, and saleable zinc ore left behind. The expense of roasting must have been considerable, but as the calamine ore was always roasted in any case before export, to lower the freight charges, it was not so great as it seemed.

Mr. Claude Vautin: The separation of iron from blende is a subject of very great interest, and I think the work lately done by Mr. Blake in the United States throws a great deal of light on it. Although the subject of magnetic separation is remarkably fascinating, I scarcely think the results obtained by Mr. Lawrence justify anyone saying that it is a complete success. I absolutely disagree with Mr. Rickard as to the quality of

centration, whereby most of the galena is separated. Then a mixed concentrate of blende, galena, and iron pyrites is calcined (not with the same object as Mr. Collins referred to, but with the object of converting the iron into ferric oxide). The product from the furnace is then re-jigged, and the result is that they get a perfectly clean blende, the oxide of iron being so easily removed. Should any blende and galena be attached to each other, they become separated in consequence of the calcination, but without in any way altering their individual character. Tho result of this process is that in the States they have been able to turn into cash large dumps containing blende, and there are now a number of mines working which, probably, a few years ago would not pay at all. I received about two months ago a copy of the *Transactions* of the American Institution of Mining Engineers, entitled "The Separation of Blende from Pyrites—a New Metallurgical Industry," and I advise the members here to get a copy. It will be seen they are actually able by this means to dress poor stuff containing only 20 per cent. blende up to 60 per cent. They convert the oxide of iron, which is produced in the tailings into paint. Mr. Blake also refers to magnetic separation, and the paper he has written is a most suitable reply to the paper which has been read by Mr. Lawrence. I think before any work is done on the magnetic separation of iron from blende, Blake's process ought to be tried. The whole secret of the success of his system is the perfect oxidation of the pyrites without altering the character of the blende or galena by not depending so much on heat, but by simply making use of the large amount of unburnt hot air by the use of Siemens regenerators. The result is that in the furnace he has used for the last three years they are treating 20 to 25 tons a day, and getting a product which is exceedingly valuable. I sincerely hope that all interested in the dressing of blende ores will try the Blake system before adopting any other method.

Mr. J. H. Collins said that one of the earliest and most successful workers in the magnetic field was Mr. King, whose machines had been in operation for a good number

of years, and who used permanent magnets instead of electro-magnets, which he thought were considerably cheaper in working. He did not think Mr. Vautin's remarks bore out the conclusion he announced. The fact of the zinc only being raised to 48 per cent. was no evidence that the separation was not a very good one. There was zinc blende which would assay about 64 per cent. of zinc, and an equally pure crystallised blende without one atom of gangue about it which would not assay more than 38 or 40 per cent. This latter was a ferriferous blende, which was known by the name of Christophite, and however perfectly it was purified from foreign matter a high percentage could not be obtained. If Mr. Lawrence would allow him, he would take exception with regard to one or two of his verbal expressions. He had spoken of "magneto pyrites." Why should he not say magnetic pyrites? There were magneto-machines, but magneto-pyrites was a new word, and would it not be as well not to make any further use of it? Then he applied the word "pulp" to stuff which had been crushed between roll crushers, and which had not passed through the stamps. He would suggest that it would be better to reserve the word "pulp" for stuff which had been treated by stamping, and not apply it to stuff where the grains were of a considerable size, such as were obtained from the jigger. Mr. Lawrence had also suggested that the magnetite which con-

Mr. VAUTIN asked if Mr. Lawrence could give him a rough assay of the material marked "A."

Mr. RICKARD said he would further like to ask Mr. Lawrence whether the concentrated material contained any phosphorus. If not, it would be a curious coincidence, and one which would be very much in favour of the machine—namely, that all the iron ores which have been dealt with by the machine seem to have been entirely clear of phosphorus.

Mr. LAWRENCE then replied as follows: In answer to Mr. Garland's remarks, I may say that the galena associated with these poor ferruginous blende ores contains only some 27 ounces of silver to the ton. As stated in the paper, the principal products of these mines were galena ores, carrying in the rich payshoots, 40 ounces of silver to the ton. The galena from the poorer shoots of lead ores contains about 30 ounces to the ton. The ores at Pierrefitte were separated into three main classes—viz. (1), Lead ores, containing 6 to 12 per cent. of galena, and no, or very little, blende; (2), rich blende ores, containing 1 to 2 per cent. of galena, free from magnetite; and (3), poorer blende ores, intermixed with magnetite. I have not found any loss of silver resulting from water-dressing these ores. I take it that the silver in the galena is in alloy with the sulphuret of lead, and it is difficult for me to understand how silver can be lost without losing the lead also, unless there are free sulphurets of silver intermixed with the galena. Sulphuret of silver, or argentite, although of high specific gravity, has, so far as I am aware, not yet been successfully concentrated by water-dressing, on account of the very minute grains this ore is generally found in. In answer to Mr. Rickard's query, whether by more continuous and modern machinery the price of dressing might still be further reduced, I may say that an improvement in this direction could no doubt be made by working on a large scale. On account of the transmission of power, and the general arrangement of existing buildings and machinery, we were not able to give these works as much fall as I should have liked to have had for the most economical handling of the

various products, and some extra handling might possibly have been avoided under more favourable circumstances. The capacity of these works is 20 tons in 12 hours. We could not make them of larger capacity on account of having no more water to spare, nor could we transmit more than this quantity of ore daily from the mines without curtailing the output of other ores. The volatilisation of zinc process I am afraid would not be advantageous here, as the cost of fuel and transport is high. There would also be great difficulty on account of the fumes, necessitating the purchase of large tracts of cultivated land. I have not heard of any mineral other than magnetite and siderite being treated magnetically, and have listened to Mr. A. L. Collins' remarks with great interest. When no middlings were made in the separator, we found it impossible to make our blende higher than 40 per cent. of metallic zinc without excessive loss of blende. Our smelter did not care to buy under 45 per cent. These middlings consist of particles containing mixed blende and magnetite, and require further disintegration to utilise them. They assay from 25 per cent. to 27 per cent. of zinc. There is no phosphorus in this ore. In answer to Mr. Vautin's remarks on Mr. Blake's achievements in Wisconsin, U.S.A., it must be remembered that Mr. Blake is dealing with blende and iron pyrites, and not with magnetite, as we are. To turn the magnetite into another oxide of iron, which would be less

metallic zinc. Zn_2 Fe S_3, or Zn_3 Fe S_4 is most likely the nearest formula for this mineral. In answer to Mr. Vautin's remarks, I may say that although the removal of the ·40 per cent. of sulphur in the magnetite might be more difficult than I am at present prepared to admit, the difficulty ought not to be insurmountable. By making more middlings the contents of sulphur in the magnetite can be all but eliminated.

Mr. Arthur Dickinson then read the following paper :—

ON THE ASSAYING OF ARSENIC ORES.

By Arthur Dickinson *(Student)*.

The form in which arsenic is most widely used as a commercial article is that of White arsenic—As_2O_3. It is prepared by calcining the arsenical ore in a reverberatory furnace or in an Oxland or Bruckner Calciner, by which operation the arsenic is volatilised and caught as white arsenic in long condensing chambers. Arsenic ores are found in greatest abundance in the district lying between Tavistock in Devon and Callington in Cornwall, and the mines which have been most celebrated for its production are the Devon Great Consols and the Holmbush Mine. The principal mineral from which arsenic is obtained is Mispickel FeS_2 + $FeAs_2$ with 46·6% of arsenic, usually called Arsenical Pyrites, and known locally as Mundic. Copper Pyrites is often associated with it. The author was recently called upon to study the various methods of assaying the ores, and to find out a method for every

scientific. The difference in weight does not actually represent the amount of volatile matter (arsenic and sulphur) present in the ore, seeing that the substance after calcination consists of *oxide* of iron and gangue, the iron having been oxidised during the process. Then again, the sulphur present is very variable in amount, and it is impossible for the operator to be always correct in his deduction. The following examples will show how great a difference there may be between the results obtained by dry and wet methods:—A rich sample containing but a very small percentage of sulphur, was found by the dry method to contain 32·2 % of white arsenic; when assayed in the wet way it gave 30 % of arsenic or 40 % white arsenic, the difference in this case amounting to 7·8 %. In some cases, however, where much sulphur is present, the dry method will give too high a result. In the following instance of a pyritic ore, the results by the wet and dry methods respectively were 9·1 % and 14 % of white arsenic. There is, in fact, no fixed ratio between results obtained by the two methods. To determine the absolute amount of arsenic, there is no doubt that a wet method of assay is preferable. There are three principal methods for the assay of arsenical ores:—

1. The Gravimetric Method.
2. The Iodine Method.
3. The Uranium Acetate Method.

In the Gravimetric method the arsenic, which is in solution as arsenic acid, is precipitated by magnesia mixture, and the precipitate weighed after ignition as Pyrarsenate of Magnesia. Unfortunately, it is necessary after the addition of the magnesia for the mixture to stand at least 12 hours before filtering, and therefore this method cannot be employed where rapidity is required.

In the Iodine method the arsenic is determined in the lower state of oxidation, the chemical reaction that takes place on addition of the standard solution being represented by the equation $Na_3AsO_3 + 2I + H_2O = Na_3AsO_4 + 2HI$. For arsenites and arsenides the method

is admirable. It has, however, proved to be of no practical value in the case of arsenical pyrites. The ore is soluble only in nitric acid, and there is great difficulty in obtaining the arsenic in a right state for titration—that is in the lower state of oxidation. Many experiments were made with a view to working by this method. It was found that even after digesting for many hours in the Ferric Chloride Mixture solution was only partially effected. Again, an attempt was made by boiling with the Fe_2Cl_6 mixture, after lowering the boiling point by dilution with water to prevent escape of $AsCl_3$, but without success. The ore was then digested for some time with Fe_2Cl_6 dissolved in dilute HCl, the $CaCl_2$ dissolved in HCl being added; but again the results were unsatisfactory. Many attempts have been made to fuse the ore with fusion mixture, and it was found that however slowly and carefully the operation was conducted, the arsenic always came off before fusion was effected.

It has been found that the third method—that in which the Uranium Acetate is employed for titration—is the only satisfactory one in the case of Arsenical Pyrites. In this method the arsenic is determined in the higher state of oxidation. The following method of assay was adopted: Solution was effected according to the method given by Beringer. The ore was first treated with nitric acid, and then evaporated to a pasty condition; the residue mixed

perfect separation of the iron present is effected—and, of course, Ferric salts, which would require their equivalent of arsenic, must be completely removed. The method of taking to pastiness with HNO_3, making up with water and dilute ammonia, and passing sulphuretted hydrogen through for some minutes, is not satisfactory. The results were in all cases low. Using, however, the fusion mixture rapid and accurate results can be obtained. Though the balance used was of a simple description, and conditions not favourable to very accurate working, results on the same sample rarely differed by more than ·5 %. The whole operation can be completed in half an hour, and with a muffle for the fusions any number of assays can be conducted at the same time. The Uranium Acetate is certainly an expensive salt —at the present time it can be obtained for 2s. 6d. the oz. The strength of the solution requires 34·1 grammes of the salt to 1 litre, in which case 100 c.c. would be equivalent to ·50 grammes of arsenic. Working with ·5 grammes of the ore, 30 c.c. of the solution will be the maximum required, and the maximum amount of the salt required for an assay will be 15 grains, which is equivalent to rather less than 1d. The method is, in fact, very little, if at all, more expensive than the Iodine method.

DISCUSSION.

Mr. Benedict Kitto said the assays by the Uranium method were applicable for ores such as the author of the paper had been working upon, and for arsenic in other ores and metallurgical products. He had used it in his laboratory for years, where hundreds of arsenic assays had been made by the Uranium method. He thought it a very great pity that assays should ever be attempted where the assayer had only a rough balance for use; and he did not think duplicate arsenic assays coming to within ·5 of each other should be considered too accurate. Regarding the

cost of the Uranium acetate, years ago he had paid 2s. 6d. and 3s. per oz. for that salt, but would not like to pay that amount now. At the present time he made his own uranium acetate and at a very cheap rate.

Mr. D. A. SUTHERLAND: I should like to endorse what Mr. Kitto has said with reference to the use of the rough balance, &c. If I may seem to speak in any hostile spirit, it is only because I object to processes being given which are not accurate within ·5 per cent. Of course, for rapid work done at mines I admit that there is value in such processes. There is no doubt that many a time in dressing ores it is of very great advantage to be able to determine very rapidly the percentages obtained in the various products. At the same time, I think the author of the paper might have given us some more details in order to enable us to form a more definite idea as to the experiments carried out. For example, he dismisses the gravimetric method very summarily, and I might even remark that by the use of molybdate of ammonia and an intermediate precipitation, the twelve hours settling may be diminished to half an hour. Some two or three years ago I had occasion to examine, in as short a space of time as possible, a very large number of samples for arsenic, and I found that the gravimetric method was extremely satisfactory and fairly rapid. Further than this, it does not require the same delicacy as that involved in the volumetric

results without great care in the manipulation and following closely several precautions, not the least important of which is that the solution must be invariably titrated when boiling. I can only express my regret that the author has not given us a series of results; and I do not think I can add further than to say that I should like to see in all papers of this class a careful note of the more important points to be attended to with the effect on the results, if such precautions are not accurately carried out. I do not know a better book for practical men engaged in mining than the one quoted by the author—namely, Beringer's "Assaying."

Mr. W. F. Wilkinson: As I have been associated with Mr. Dickinson in the experiments which he has described in his paper, I should like to make a few remarks. I may remind the members that although arsenic ores belong to the less common or useful minerals, they are of considerable importance in the mineral production of Cornwall; in fact, I believe that the greatest part of the arsenic of commerce comes from the mines in Cornwall, and it is therefore surprising that so absolutely unscientific and inaccurate a method of assaying as the dry assay should still be the ordinary method used in the district. Mr. Dickinson made experiments with the wet methods, and found that the uranium acetate process gave rapid and accurate results. Considerable difficulty was met with at first in the preparation of the solution. In fusing the powdered ore, when rich in arsenic, with fusion mixture, a loss by volatilisation nearly always took place. This difficulty was overcome by treating the ore with fuming nitric acid and evaporating to dryness before fusion. I have not seen any text-book in which this method of preparing the solution expressly for the uranium acetate process is described, though it is fair to say that Beringer's well-known book gives the method under general instructions for preparation of solutions for wet methods. With regard to the balance used in these experiments, it is, of course, to be regretted, for scientific reasons, that results could not be obtained nearer than a-half per cent. For practical purposes, however, the results

obtained were sufficiently accurate. If closer results are required, I have no doubt that, with a good balance, they could be obtained. It may be of interest to the members to know that similar experiments in assaying arsenic ores were made many years ago at the Devon Consols mine by Mr. Richard Pearce, an old School of Mines man, and now the distinguished metallurgist and manager of the Argo smelting works, Colorado. I am indebted to a gentleman who worked with him for valuable information on the subject, especially in connexion with the preparation of the solution. Mr. Pearce read a paper on the estimation of arsenic in ores before the Colorado Scientific Society in 1883, in which he advocated the nitrate of silver method. Particular attention is drawn in this paper to the losses that may occur by deflagration in fusion, unless the precaution is taken of first oxidising the arsenic and sulphur with nitric acid. As the Proceedings of the Colorado Scientific Society are somewhat difficult to procure, I think Mr. Dickinson has done great service to this branch of mining by bringing this paper before our Institution.

Mr. Henry G. Graves: Mr. Kitto has told you that he has made hundreds of analyses of arsenic in ores and metallurgical products, and I had the pleasure of being in his laboratory and assisting in doing many of those some years ago. We used the Magnesia Mixture Method, and the Uranium Acetate Method, as used by Mr. Dickinson,

[redacted]

at a mine. We cannot expect to carry out such processes as they can be carried out in the laboratory, and if this process is a simplification, then we ought to thank the author for the work he has done. I think myself that he will probably be able to simplify his process still further so as to make it valuable for the class of men who have to work it out. There is no doubt whatever that we want better methods of estimating rapidly and simply the substances with which these men have to deal. It can be very easily dealt with later on when submitted to a proper analyst who has the proper appliances; but at the mine or in a metallurgical work we want to get some means of telling rapidly and reasonably closely what we have to deal with. I think, therefore, we ought to look upon this paper as a paper of that class, and not as a paper which professes to be one dealing with minute chemical accuracy.

The President: I quite agree with Mr. Mactear in the remarks he has just made. It is, of course, most necessary that there should be a rough and ready method of finding out the average value of the ore, which can be used at short notice; and for that reason I think there should be, as a rule, a distinction between the miner and the metallurgist. The miner should be able to know what he has in his ore, and return approximately what the contents of it are, and, if necessary, to go further than that; but it is impossible for a man who is a miner, and who is engaged for the major portion of his days in attending to mining operations, to be as good an assayer as a man who devotes the whole of his time to it. Therefore I think our thanks are due to Mr. Dickinson for the trouble he has taken in pointing out the different processes for the assaying of these arsenic ores, which after all are limited to a comparatively small area. I will now ask him to reply to the remarks which have been made upon his paper.

Mr. Arthur Dickinson: First of all, in reply to Mr. Kitto's remark about the balance not being of a very accurate description, I ought perhaps to have stated before that the experiments made by the iodine method were

carried out at the South Kensington Metallurgical Laboratory, where, of course, one has very good balances, and it was only after I got down to Cornwall that I was handicapped by having a rough-and-ready concern. I am very glad to hear from Mr. Kitto that I have been paying too much for the salt. I hope I shall be able to get it cheaper in the future. Mr. Sutherland asked about the details of the process. A great many of the details are given in Mr. Beringer's excellent book on "Assaying," and I thought I had given as many as were necessary in my paper. As I have shown in the paper, the Cornish method was found to be most unreliable and unsatisfactory, and I therefore made experiments with a view to determine a method of assay for the class of ore in question, which should be rapid, easy, and fairly accurate. I say "fairly accurate" advisedly for a remark has been made to the effect that it is not nearly sufficient that results on the same sample should differ in some cases by ½ per cent. Such a difference is quite permissible for the assay of arsenic ores in the mine laboratory, and a greater degree of accuracy is not requisite. The importance of obtaining a suitable method of assay was made very evident to me by an instance that came under my notice when in Cornwall. I was told that during the last working of the mine a lode was discovered that to all outward appearances was rich in arsenic. An assay made by the old method showed that it should contain 20 per

cent., or even to 1 per cent. I have found the uranium acetate method to satisfy all the requirements, for it is easy to work, is rapid, and very fairly accurate.

FOURTH ORDINARY MEETING.

THE FOURTH ORDINARY MEETING of the Third Session was held at the Museum of Practical Geology, Jermyn Street, London, S.W., on the evening of Wednesday, February 21st 1894, at 8 o'clock.

Prof. A. K. HUNTINGTON (Vice-President) occupied the chair.

The minutes of the preceding meeting having been read and confirmed, and the Secretary having announced the names of gentlemen recently admitted to the Institution, the Chairman called upon Mr. Joseph Garland to read the following paper :—

[redacted]

ON NICKEL MINING IN NEW CALEDONIA.

By Joseph Garland (*Member*).

The discovery of nickel ore in New Caledonia is by common consent due to M. Jules Garnier, who, when engaged in a geological survey of the island in 1864, met with the strange green mineral which was destined some years later to become one of the most important sources of nickel in the world. M. Garnier made known his discovery three years later in a memoir* published in the *Annales des Mines* for 1867. It was not, however, till 1874 that the discovery became the object of serious attention. In that year the first attempt at mining the nickel ore was made at Mont d'Or, a conical and conspicuous peak a little to the south of Noumea, the capital.

At first there was a good deal of misconception as to the true nature of the mineral, and for a time its green colour gave rise to the belief that it was an ore of copper; and, indeed, a shipment was, it is said, made to a smelting works in Sydney to determine its value for that metal. It is needless to add that the attempt to extract copper from the ore proved futile. It is not, however, the first time that an ore of nickel has been mistaken for copper ore, for it is well known that *Kupfernickel* was so called by the German miners because they frequently mistook it for an ore of copper.

It is recorded that when the French colonists realised the importance of the discovery, the government offices were daily besieged by applicants for concessions, and that the excitement became intense.

* "Essai sur La Géologie et les ressources Minérales de la Nouvelle-Calédonie."

The deposits at Mont d'Or were found to be unremunerative, but it was not long before fresh discoveries were made in other parts of the southern half of the country along the east coast, and mining for nickel soon became an established industry.

For the first few years the mines were worked solely by private enterprise, but in 1881 fresh impetus was given to the new industry by the starting of a big French company, "La Société le Nickel," locally known as the "Nickel Company," which, though not the only nickel mining company in the island, is by far the most important. It has acquired mining rights over an immense area; it works a large number of mines under the direction of its own officers, and leases a large number of others; it is the purchaser of all the nickel ore raised in the island; has a monopoly of the shipment of the ore to Europe, and has, moreover, enormous influence in the country.

It was for some years believed that the nickel-bearing formation was limited to the mountain ranges along the east coast, and it is along this coast that the chief mines are still being worked; but it is now an established fact that the magnesian rocks in which this metal occurs have a very wide extension also on the west coast, and a number of valuable mines are now being worked on that side of the island.

The author found during his recent visit that the

Geological.

It is not the object of this paper to discuss the general geology of the island: that would be outside the author's opportunities of study and personal observation, and, moreover, would not conduce to the elucidation of his subject.

Broadly speaking, the southern half of the island, with which this paper deals, consists of serpentines and schists; but the former is the most prominent formation, and it is solely in this rock that the nickel deposits occur. It must not be understood that this rock is always and everywhere the repository of nickel—that is not so; but wherever the ore is found, it is invariably in the serpentine.

This rock is the common massive variety, of variable colour, dark green and greenish grey being, perhaps, the predominating shades. It is sometimes traversed by veins of steatite, and often exposes steatitic faces, which have the smoothness and unctuous feel characteristic of soapstone; outcroppings of asbestos are also occasionally met with.

It is not, however, veined and mottled like, for instance, the serpentines of the Lizard; indeed, the noble or precious serpentine was nowhere observed by the writer, though he travelled over a great extent of serpentine country; nor could he find, on enquiry, that any variety of the rock was ever used for decorative or ornamental purposes.

The serpentines form whole mountain* ranges, running into peaks and ridges of considerable height; and it is a noteworthy fact (which the author will leave to the geologists present to explain) that the nickel deposits occur almost invariably in elevated positions, often at the very summits of the mountains. Indeed, a visit to a New Caledonian nickel mine, involving, more frequently than not, a climb up the very steep sides of a mountain some 600 to 1,000 metres in height, in a tropical sun, is an experience well calculated to impress its novelty upon the mind of the visitor.

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* The country is extremely mountainous, the central chain, north to south, attaining a height of 1,700 metres (5,576 feet) and averaging 500 metres (1,640 feet), whilst the mountains along the coast are from 100 to 200 metres in height.

As a general rule, the nickel-bearing summits are capped more or less extensively—sometimes very extensively—with ironstone. These deposits are often several feet in thickness, and consist of a confused mass of boulders and rounded pebbles, from the size of the largest cannon ball down to that of small shot: underlying the rounded lumps of hematite, and sometimes extending for some distance down the slopes, layers of oolitic gravel of the same material not unfrequently occur. It is stated on good authority that the ironstone carries a little Chromium to the extent of 4 to 8 per cent. of the sesquioxide, Cr_2O_3.

The ironstone is much weathered, is quite black, and very vesicular.

Another conspicuous feature is the frequent occurrence on the serpentine slopes of a peculiar red earth, often in patches of considerable extent, and appearing from a distance like huge scars. It is generally spoken of as argillaceous ironstone, or ferruginous clay, but the author gathered from an excellent chemist on the spot, to whom he is indebted for the following analyses, that it contains practically no alumina; it consists almost entirely of iron and silica, with 1 to 3 per cent. of nickel, and has no commercial value. The analyses referred to are :—

	I.	II.
SiO	18·42	19·45

and to a moderate depth, and in these cases partaking of some of the characteristics of a regular lode. Veins occur, for example, 2 to 3 feet in thickness, with good regular walls and a regular dip, and taking at times a lenticular form 4 or 5 feet in thickness. It is generally held, however, by the French engineers that these veins are not true fissure lodes.

The lesser veins and stockworks could not be profitably worked by underground mining, and are invariably exploited in open quarries. It is, I believe, an established fact that there is a certain small proportion of nickel in the serpentine rock—viz., 0·25 % to 0·75 %.

Mineralogical.

The mineral in question is a hydrated silicate of nickel and magnesium. The arsenides and sulphides are not met with in New Caledonia. Of this double silicate there are two distinct varieties, the green and the brown.

Some mineralogists recognise two varieties of the green ore, and, following Professor Liversidge, designate them Garnierite and Noumeaite respectively; the term garnierite after Garnier, the discoverer of nickel ore in the island, and noumeaite from Noumea, the capital within a few miles of which the first deposit was found.

Describing garnierite, Liversidge says, in a paper read before the Royal Society of New South Wales in 1880:* "It is at once distinguished from the more important mineral noumeaite by its adherence to the tongue, and by its falling to pieces when immersed in water, and even when allowed to remain adherent to the tongue for a moment or so. . . . . Apart from these characters, there appears to be but little difference between the two varieties."

The author, after spending several weeks in the nickel district, visiting a number of the most important mines, and studying the deposits and the character of the mineral,

* "Notes upon some Minerals from New Caledonia." By A. Liversidge, Professor of Geology and Mineralogy in the University of Sydney.

ventures to doubt if there are any sufficient grounds for the distinction here drawn—a distinction without a difference, he is inclined to think, and one, he believes, never recognised by the mining engineers and chemists on the spot. Not only is there, in his opinion, no justification for the sub-division into garnierite and noumeaite, but he is decidedly of opinion that it is only common justice to the discoverer to designate this mineral garnierite, understanding by that term the green silicate of nickel and magnesia, without reference to the various shades of colour it assumes, or to the percentage of nickel it may contain.

This New Caledonian mineral is always found amorphous—never crystallised. It occurs in massive pieces, in botryoidal, mammillated, and occasionally in stalactitic forms, and in brecciated masses. Imbedded in the mineral are often angular lumps of serpentine rock.

It varies greatly in hardness, sometimes being quite soft and brittle, crumbling between the fingers, and in other cases hard enough to be cut into shape and to take a fair polish. In colour there are very pale tints of green (which appear to be only silicate of magnesia with a mere trace of nickel, and of no value), apple greens, and on to a rich dark green—the darker greens being generally richer than the pale green ore. Its specific gravity is about 2·5. Its composition is very variable, and the fact that no two formulæ that one finds in text books and museums scarcely ever

has sometimes the following composition: silica, 45; nickel, 26; Magnesium, 13; iron, 3; water 13.

As to the brown variety of the mineral, it has practically the same composition as the green ore, but is understood to contain a little more iron, a little less magnesia, and to be somewhat richer in nickel than the green ore. It is always spoken of in the district as "chocolate nickel" or "chocolate." This variety has not been so long known as the other. The author may here venture to quote an extract from an article contributed by him, in October 1892, to the *Sydney Morning Herald*: "The green mineral has been mined since 1874; but not till about 1887, or the following year, was it recognised that the brown variety was an ore of nickel, and seeing that it occurs in veins, joints, and fissures, traversing the serpentine rocks precisely in the same manner as the green ore, and often intimately mixed with it (though also occurring in separate veins), there is no doubt that prior to that date much valuable brown ore was, in ignorance of its worth, thrown away to the spoil heaps. The chocolate ore, like the green ore, varies very much in quality, there being a light brown and inferior ore, just as there is a light green and low-grade ore; but, speaking roughly, the chocolate nickel is understood to be the richer of the two; and that this is a generally recognised fact is evidenced to anyone who moves about the island for ever so short a time, by the eagerness with which a discovery of 'chocolate' is hailed. There is a distinct similarity in the composition of the green and brown varieties, in that they are both hydrated silicates of nickel and magnesia, the brown containing a larger proportion of the peroxide of iron and less magnesia. Speaking generally, all the nickel mines of New Caledonia yield the green nickel ore, but in some mines but little of the brown ore is met with, while in others the chocolate variety forms quite a distinctive feature. In conclusion, as the term 'chocolate nickel' has become grafted into the mining phraseology of the district, I would venture to suggest that a concise and distinctive mineralogical name for the brown hydrated silicate of

nickel and magnesia, or chocolate nickel ore, would be '*chocolite.*'"

Through the courtesy of Mr. Arthur C. Claudet, our excellent Treasurer, the author is able to give an analysis of a specimen of the chocolate ore which he submitted to him for examination. Mr. Claudet writes:—

> I may say that this particular ore consists chiefly of an almost intimate mixture of two minerals—viz., hydrated silicate of nickel and magnesia, and hydrated oxide of iron, the latter probably limonite. The specific gravity of this ore I find to be 3.000, the presence of the limonite raising it above that of garnierite (hydrated silicate of nickel and magnesia). I send you three small specimen boxes containing a small piece of the ore in its original state as received by me, a small piece of the ore treated by dilute hydrochloric acid, which dissolved the limonite leaving the green mineral garnierite, and some of the ore treated by the same acid after the ore had been finely pulverised. The following is the composition of the chocolate nickel ore:—

		Nickel.
Oxide of nickel	12.25 °/.	= 9.64°/.
„ „ iron	32.20	
Magnesia	3.07	
Alumina	3.62	
Silica	34.80	
Water, at 212° Fahr.	6.48	
Water above 212° Fahr.	7.07	
	99.44	
Specific gravity	3.000	

[redacted]

of good width; the latter method when they are small—say less than from 6 to 12 inches—and when stockworks occur.

It sometimes happens that both systems come into operation in different parts of the same deposit. For example, the larger veins are exploited by tunnels, and subsequently the cap or top of the peak is quarried for the sake of the numerous small veins.

When underground mining is resorted to, the system followed is that universally practised in metal mining, and familiar to all mining men.

A series of tunnels or adit levels, one below another, are extended into the mountain along the vein, and connected in the usual way by winzes and rises for ventilation, and for convenience of stoping. An example may be cited. The mine is situated near the top of a steep mountain, close to the Bay of Nakety. The outcrop of the main vein at the summit is some 1,300 feet above the sea. Large masses of loose ironstone cover the plateau. The vein has a regular strike, and has been followed into the mountain for some 100 metres; it is bounded by good walls, which have a dip of about 45°; its normal width is 2 to 3 feet, but it is about double this in places. The enclosing rock is a dark green serpentine. The ore is of excellent quality, consisting of the green and brown varieties—garnierite and "chocolite" (if the latter term may be permitted)—in the same vein, the latter rather predominating, a fact at which the proprietor—a hearty, hospitable and enthusiastic Frenchman—is naturally highly gratified. The workings had attained a depth of about 350 feet from the outcrop, and at the deepest point reached the vein was still 18 inches in width of rich ore. The mine had been at work for about two years on a very moderate scale, and had yielded 2,500 tons of ore, averaging 7 per cent. of nickel.

Very brief mention may be made of one other nickel mine visited by the author, where the bulk of the ore was raised by open quarrying, but where there was also some underground mining of a somewhat irregular kind. The veins, some of them several inches in thickness, were not persistent in any one direction, and thus it happened that after

driving on a vein a few yards it might be cut off by a productive cross vein; the latter would then be followed right and left till another interruption occurred or the vein became poor. A winze would then be sunk or a rise put up to prove the vein, and perhaps fresh levels driven in different directions from the bottom of the one or the top of the other; and thus the mine, to a visitor, seems to be a perfect labyrinth.

Quarrying is done in terraces from the top downwards, each terrace having a wide floor on which the rock is blasted down, and where the ore is prepared for market.

Each quarry has a face of nearly vertical rock, say, of 10 to 15 metres in height, and a floor of, say, 20 to 40 metres in width. Deep holes are drilled by "jumpers" in the face of the quarry and blasted with dynamite.

The rock being very jointed, it breaks down in large lumps or blocks, which have subsequently to be reduced by heavy hammers or shot-holes to a convenient size for setting free the veins and strings of ore, and for easy removal of the dead rock in waggons or barrows.

Roughly speaking, every joint and fissure of the rock is filled with ore, the veins thus ramifying in every direction varying in size from, say, a knife-blade thickness up to 6 or more inches, but the smaller veins are the most common and numerous, and hence, to secure all the ore, it becomes necessary to blast down the whole mass of rock—this being

removed. The picked ore is now thrown with the fines and the cobbings, and the whole mixed together forms the marketable product.

As might be expected, the ore prepared in this crude way, by screening and hand separation without the aid of water, contains a large proportion of stone and dirt; but this is the system universally adopted throughout the nickel mining district, the difference in the specific gravity between the nickel ore and the country rock not being sufficient to make easy and economical separation by water practicable.

The average quality of the ore shipped to Europe is 7 to 8 per cent. of metallic nickel. No ore of less than 6 per cent. is considered marketable, nor will the Nickel Company purchase ore of a lower grade.

The cost of mining and preparing the ore is one which the author must pass over very cursorily. It naturally varies according to whether the deposits are mined or quarried; it is affected also materially by the distance inland, the height above the sea, and by the conditions of labour. The author has heard of cases where the cost was so low as 6s. per ton, and of other cases where it reached 40s. per ton.

As to the class of labour, it consists, in the main, of convicts and ex-convicts. Of the available convicts—*i.e.*, those whom the Government can spare from the public works—the majority are employed by the Nickel Company.

The ex-convicts, or *Liberé*, are men whose term of enforced servitude has expired, and who are now free to move about the island as they please, but who are exiles for life. These men, who find no difficulty in getting employment, command a wage of about 5 francs per diem at the mines.

The native Kanakes, who form the bulk of the inhabitants, seldom work in the mines if they can avoid it, and their wants being few, they manage to a great extent to live without work. Indeed they are said to hold strong views as to the folly of work, and the stupidity of the white man who is addicted to it. The Government having made them all sober by Act of Parliament—no one being allowed

under heavy penalties to sell them any beverage of an exciting nature—they have only to work a few days now and then to purchase tobacco, or to replenish their very scanty wardrobes.

The scarcity of labour is one of the chief difficulties in connexion with mining in New Caledonia, and Kanakes are imported from the neighbouring islands, and even Chinese and Japanese are sometimes imported in considerable numbers.

It is a novel and interesting experience to see a big mine or a series of quarries in full work, manned entirely by convicts, guarded by a number of warders all armed with revolvers. Some of the men drilling holes, others blasting with dynamite, some spalling, cobbing, screening, or hand picking, whilst others are tramming or wheeling away the waste rock. Some also are placing the ore in bags ready for transport down the mountain side, on cable lines or tramways, to the wharf for shipment. All is activity, but there is an absence of zest and cheerfulness in the performance of the task, for, naturally enough, the convict is not notorious for the energy and enthusiasm he throws into his work.

At a group of four mines at Thio, owned by the Nickel Company, which the writer was courteously allowed to visit, 900 men were employed, 750 of whom were convicts, and 150 *liberé*. The latter are not employed at the same mines

Single cable tramways are sometimes used for short distances. They are simple and inexpensive, but of limited application.

A single wire rope is anchored at each end in the usual way. A tough forked stick takes the place of the pulley; one leg of the fork is pierced with a hole, from which a stout wire, bent into a circular form to carry a bag of ore, hangs suspended. A number of these rude suspenders are kept ready for use.

When it is desired to send the ore down to the depot, one of the forked sticks is placed across the wire rope, a bag of ore is simply balanced (not fastened) in the wire loop, a push is given, and the load goes spinning down the cable at great speed to the terminus, where it bumps against a battery of filled bags and is then removed.

Only one bag of ore is sent down at a time, but they are generally made to follow each other very quickly. If a load happens to stick fast in mid air, another cargo is despatched with an extra push to dislodge it; this generally succeeds in inducing the truant to move on, but occasionally the concussion disturbs the balance of things and the bag falls to the ground, the contents being scattered by the impact.

This is a rough and ready method of sending down ore from new points and almost inaccessible places; but it involves carrying up the empty bags and forked stick suspenders—if they may be so designated—to the working points on men's backs.

The author regrets that, owing to an entire discontinuance of smelting operations in the island, he had no opportunity during his recent visit* of studying the metallurgical side of the subject.

He gathered on enquiry that smelting of the nickel ores into matte had been attempted on two or three occasions, and that a few thousands of tons of matte, of 50 to 70 per cent. metallic nickel, had been obtained; that coke

* In the autumn of 1892.

(imported from Sydney) was the fuel used, the flux being gypsum, or its substitute, limestone and sulphur.

For some reason, however, smelting operations have been discontinued, and the ore is all sent to Europe; the bulk, if not the whole, it is understood, to Glasgow, to be metallurgically treated.

In conclusion, it may be mentioned that the nickel deposits of New Caledonia are said to extend only to a limited depth, one or two formerly important mines having, it is reported, ceased to yield nickel at a depth of 300 feet, the ore at that depth passing into the white silicate of magnesia. The author has no evidence which would justify him in venturing a decided opinion on this point, but the enormous area which the nickeliferous serpentines cover seems to indicate that it will be a very long period before the nickel deposits of New Caledonia will become exhausted.

DISCUSSION.

Mr. S. Herbert Cox: I have not been in New Caledonia, but there are many points in Mr. Garland's paper which have struck me as of very great interest, and more especially, so far as I am concerned at any rate, in the

point to me that the characteristics of the rock in New Zealand appear to be almost identical with those in New Caledonia, and yet the mineral contents of the New Zealand rock is quite different—that is to say, that instead of nickel occurring in the New Zealand beds, copper occurs, and it seems to occur under very similar conditions. The lodes of copper there are irregular lodes—perhaps more properly called lenticular deposits, as Mr. Garland says. They have certainly a very close resemblance to lodes in certain places, although pinching out and widening in a very irregular manner; and they run from copper to this hydrated silicate of magnesia, which Mr. Garland speaks of. There is one point I should like to know very much, and that is whether in New Caledonia these nickel deposits which die out in depth and pass into this silicate of magnesia have ever been followed down sufficiently far to see whether the same lodes open out again, and other deposits of silicate of nickel occur below. I think it would also be very interesting to know if Mr. Garland can tell us whether copper occurs with the serpentine, and in lodes near these nickel lodes, because it strikes me that there must be some sort of connexion between the two classes of deposits, the conditions being so similar in the two different places. Then, as regards the nomenclature of the nickel ores, Mr. Garland very properly has begun his paper by suggesting that we should condense the names, and that garnierite and noumeaite should be included in one group; but why, may I ask Mr. Garland, does he not go back a little further and take the name of alipite, which Dana uses, and which, as far as I can see, excepting that perhaps the silica is a little higher than in the analysis before us, appears to be identical with noumeaite and garnierite. The tendency, no doubt, is for mineralogists to spread out the names, and, instead of sticking to one name, which includes a number of species, they are very apt to run into new names on the very least provocation. I must say that I think it is rather unfair of Mr. Garland to tell us that he is going to propose "chocolite" as a new name, after suggesting in the first instance that he should condense the two other names.

The CHAIRMAN asked Mr. Cox whether the copper in New Zealand to which he had referred occurred as silicate, as, if so, it was a very interesting parallel between the conditions in New Zealand and those in New Caledonia.

Mr. Cox said it occurred as silicate on the outcrops and changed to oxide of copper and native copper, and in some parts to copper glance. He thought that these were about the only copper minerals that occurred. There was a good deal of iron associated with the copper in places, and in one case he had seen copper pyrites. Most of them were oxides and silicates but no carbonate, or only very fine incrustations of carbonate.

Mr. C. A. MOREING: I think we ought to be very pleased with Mr. Garland's paper, especially as it marks rather a fresh departure in the character of the papers we have so far had before us. It is an account by a member of our Institution of notes he has made on a professional trip, and I think a very wide field of usefulness is open to this Institution if all of us who are professionally engaged on examinations of properties of this character would make notes and communicate them for reading. If this were done we should amass a considerable amount of information which might ultimately lead us to get a proper theory as to how mineral deposits are formed. What has struck me particularly in Mr. Garland's account of these nickel mines is the very rough manner in which the whole thing seems to be

Mr. Morring: There are only 80 actual mines, but we can hardly call them mines, because they only produce 60,000 tons of nickel ore per annum. That is only averaging them at about 700 tons each per annum, which is an insignificant amount. From what Mr. Garland says some of the mines are evidently large and extensive ones, like the Thio, where some 900 men are working, so that the greater portion of the 80 must be very small affairs indeed. I see Mr. Garland throws out a hope that some geologist amongst us will explain why these deposits have a way of getting on to the top of the hills. I have often wished that deposits of all kinds had not got this unhappy way of getting on the top of mountains, but it is not peculiar to nickel mines in New Caledonia. I can quite understand that New Caledonia being very hot, and the hills being very steep, the fact impressed itself more particularly upon Mr. Garland, although I have also had it impressed upon me very unpleasantly in trying to scale the summit of the Rocky Mountains in the middle of winter. I notice Mr. Garland says in his paper that the brown variety of this mineral, which he has called "chocolite," is understood to be richer than the green variety, but the interesting experiments which Mr. Claudet has made on this brown ore seem to show that the nickel containing substance in the brown ore is simply the green ore, and that the brown substance is limonite, which is in addition to the green. It would make one think that the brown ore ought to be of poorer quality than the green. I was handling the specimens just now, and I was struck with the peculiar lightness of the brown "chocolite," and certainly to the hand it seemed much lighter than the green. I notice from the analyses given on the blackboard, that there is one case there where the brown ore is richer than the green. Of course, analyses of that sort do not prove anything, because some rich specimens of the green, and some poor specimens of the brown may have been taken. But what struck me especially was the experiment of Mr. Claudet, which is very interesting, and which seems to show that the brown ore is only an impure

variety of the green, and therefore I should naturally say it must be a poorer ore all round than the green. Like Mr. Cox, I was rather struck with the little inconsistency of Mr. Garland in christening this brown ore "chocolite," but I presume he simply means it as a useful, rough name for the ore underground amongst the miners, and not for the purpose of putting a fresh mineralogical name into our scientific books, which, I am sure, would be a great misfortune to all of us.

Mr. Thomas Rickard said he would not quarrel with the author on the subject of his new term, though rather opposed to any more than were necessary. He thought the French mining methods were neither rough nor bad, and that it would be difficult to initiate any other method which would suit the purpose so well. If the ore were of greater density, so that the refuse stuff could be eliminated, other methods could then be used. He also thought their method of transport was very good, though it was not, as Mr. Garland supposed, the first single wire rope that had been used, there having been in use for over 40 years, at the Bottino Mine in Tuscany; a single rope or rail with a contrivance in imitation of a bird's wings. The ore was sent down over this line, and the empty sacks returned on mules' backs. Mr. Garland had said that the only public company in the island was La Société le Nickel, the others being private companies. There was,

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a ton f.o.b.; whereas at Sudbury they produced their mattes and speiss and sold them up to 40 cents per lb., which meant about £160 a ton. The Canadians had hoped that the New Caledonian mines would be speedily exhausted, but there seemed to be little chance of this happening for many years to come. Then, regarding the population, he (Mr. Rickard) had learned that the natives of the Loyalty Islands were exceedingly satisfactory as workmen, and the French exploitants had brought numbers of workpeople under contract from Tonquin, who worked for two francs, forty centimes a day and their food. Mr. Garland had referred to the fact of chromate of iron being found; of this ore there was a great deal; large quantities having been exported from there. It was also important to note that while the central mass of the island was serpentine, the lower-lying grounds were composed of carboniferous rocks, from which large quantities of coal of very fair quality could be obtained. Then, as to the ore occurring on the tops of the hills, there was perhaps not much difficulty in accounting for that, as it was not the ore that had got up there, but the softer overlying rocks which had been worn down. He regretted that Mr. Garland had not furnished them with some particulars respecting the smelting operations, as it was no doubt an interesting question. The addition of flux containing sulphur was, he thought, a mistake, and one that had been given up long since. Proof of this was to be found in the fact that whereas the Sudbury nickel was got from sulphurous ores, that from New Caledonia was obtained from the silicate ores, containing no sulphur, and being smelted without an admixture of sulphur was of superior quality. This was an important condition in favour of New Caledonia. He (Mr. Rickard) thought the ferro-nickel process was now being adopted, which method was as follows: The ore was smelted in a 40 feet blast furnace to obtain ferro-nickel, which was then ground and decarbonised in a reverberatory furnace, and in subsequent treatment the iron, the silica, and the manganese were slagged off and the product sent to be refined. The refining was, however, very costly. In Canada the miner

sells his product as a matte, whereas the miner in New Caledonia sells it as 7 or 8 per cent. ore.

Mr. Percival Fowler said he would like to ask Mr. Garland if he knew of any analogous deposits to those of New Caledonia, and whether he could give any information as to the outcrops, the nature of the outcrops, and how the nickel was discovered. He would like to know whether the outcrops were iron caps or quartzose. Mr. Garland had mentioned that the enclosing rocks contained from $\frac{1}{4}$ to $\frac{3}{4}$ per cent. of nickel, and he (Mr. Fowler) would like to ask whether he has formed any theory as to the origin of nickel deposits by the segregation of particles, or whether there was any evidence as to their origin by segregation, sublimation, or deposition: also whether in any case the sulphuretted compounds of nickel had been met with. Further, whether, in Mr. Garland's observations and examinations, he had come across any of the relations which are so often found in the serpentine rocks between chrome iron or asbestos, further than mentioned in the paper; and also whether the author could give any sketch of the lenticular character, either as vein or masses of the deposits which he met with.

Mr. Cox said he would further like to ask what the gangue of the lodes was.

Mr. G. E. Collins: It seems to me that the special interest of Mr. Garland's paper arises largely from the fact that it gives us the ideas of a practical English mind upon these

Are these being worked at the present time? The occurrence of the deposits exclusively in a horizon of the serpentine, now exposed at a considerable height above the sea level is a curious fact, but I think the simple explanation which Mr. Rickard has given is a little too simple. What we really want to know is why the serpentine down in the valleys never contains these deposits.

Mr. Rickard: They are not seen.

Mr. G. E. Collins: But it has surely been tried somewhere; at any rate, they have had as good a chance of being discovered as those on the tops of the hills. It may perhaps be that the serpentine, by lateral secretion from which the nickel is doubtless derived, is only nickeliferous at or about this horizon. Can Mr. Garland adduce any evidence for or against this conjecture? Does the serpentine, for instance, contain nickel everywhere or only in the same horizon in which the deposits occur? Mr. Moreing's remarks as to the rudimentary nature of the wire tramway used are, perhaps, a trifle hard. He has himself dwelt on the small size of most of the mines, which is, of course, a result of the irregular nature of the deposits, and surely it is obvious that small mines cannot support an elaborate plant.

Mr. O'Donoghue said the tramways were moved when the boats come round, as there was a certain amount of ore brought on to the top of the hill, and which had to be carried down to the beach. They sent down 100 tons perhaps from one place, and then when the vessel moved on say two or three miles she had perhaps another 200 or 300 tons to come down. They were only little simple things which were moved from one place to another.

Mr. Benedict Kitto: What struck me most in Mr. Garland's paper, was the great dissimilarity between the nickel ore which he describes, and the ores of nickel which we get from other countries. I believe the deposits in Norway and Sweden, as well as in the United States and Canada, are arsenides and sometimes sulphides, and I suppose the only place that yields anything like the same quantity of nickel ore as the New Caledonia district

is the Sudbury district in Canada. There I believe it occurs as a sulphide in nickeliferous pyrhotite. It occurs there, as I understand, between the gneiss and diorite rocks in what are generally called contact veins, which mode of occurrence, of course, is very different from those silicate deposits spoken of in the paper. I believe the Commission appointed some few years ago by the United States Government to inquire into the quantities and qualities and methods of working the Sudbury deposits reported, if I remember rightly, that there was in sight 650,000,000 tons of this ore. That, of course, is a very large quantity indeed, and no doubt a great deal more has been opened up since that time, so that there is a very plentiful supply of nickel from that district. Then the question is whether that nickel ore can be produced at a profit. Now, the impressions that I have got, are that they have not been very profitably worked. Perhaps Mr. Rickard has more definite information on this subject than I have. I believe the deposits in the United States have not been successfully worked from a financial point of view, though I remember reading at the time that these nickel ores were said to be so mixed with siliceous matter that they could be smelted without the addition of any outside flux, so that the smelting should be cheaply done. But then we must remember that those ores contain only from 1 to 5 per cent., perhaps an average of 3 per cent. of

two-fifths of the island contained these nickeliferous deposits. A good deal of the district is no doubt too poor to be worked successfully, but there can be no doubt that the deposits are enormously large and very widely disseminated throughout New Caledonia. Mr. Cox spoke about the copper occurring in the serpentine in Australia, and that it occurred near the surface as silicate, going down into oxide and native copper. Those are just the conditions in which the copper occurs in the Lizard serpentine in Cornwall. It seemed to me rather a wrong way to go to work to convert the nickel into a matte, seeing it occurs absolutely free from arsenic and sulphur. I believe that the purest metallic nickel at present is that produced by the patent taken out by Mr. Ludwig Mond in 1890. He has treated these mattes and other sulphides and arsenides of nickel, and converted them into oxides; then he reduces with hydrogen to the metallic state, and afterwards passes carbon monoxide, which he brings into contact with the nickel. The two combine, forming a vapour. It is a rather curious property of the metal that it will combine with the monoxide, I believe the formula is $Ni(CO)_4$. This then passes into a chamber which is further heated. I believe the temperature is somewhere about 250° C. There the compound is broken up, and metallic nickel is left in a very pure state indeed. In conclusion, I have, like Mr. Cox, a very strong feeling against the multiplicity of mineralogical names, and I perhaps speak more feelingly on the subject owing to my own experience of the difficulties which have sometimes arisen in this connexion.

Mr. A. G. Charleton: The author states that the French engineers do not regard the nickel deposits of New Caledonia as true fissure veins, but he says that in places they show considerable persistency in strike (extending for several hundred feet in length to a moderate depth), possessing a good regular dip, and other characteristics of regular lodes. In another place, Mr. Garland alludes to the occurrence of patches of red earth of considerable extent, which appear in the distance like huge scars on the hillsides; and thirdly, he speaks of the nickel bearing summits.

being capped more or less extensively with a thick deposit of iron-stone and oolitic gravel; for example, near the mine he describes at Nakety.

Now one point I would very much like to know, is whether these iron-stone deposits extend along the "croppings" of the nickel-ore as far as they can be traced, and also if the author can say for certain whether they do or do not cover deposits of the red earth elsewhere exposed. If they do, it would confirm the description given by Levat, who states that the red clays, as he calls them, are mostly capped with iron-stone, underneath which the beds of cobaltiferous manganese (alluded to by Mr. G. E. Collins) are found. The point to be specially remarked is that Levat insists that the nickel deposits are found exclusively at or near the contact of bodies of this red earth with the serpentine, though never found in the red earths themselves. Now the term "scars" would give one the impression of a "basin," which it is impossible to connect with the idea of a vein several hundred feet in length, except by supposing the basin to be of elliptical shape, or that a number of small basins are ranged in contiguous rows or groups. In either of these cases, or if the red earths form as it were sheets or beds parallel with the strike of the veins, there is no difficulty in following the description given by both authorities. In that case, what are spoken of by Mr. Garland as veins, are no doubt what Levat alludes to as

to several feet in thickness. These matters are of some consequence, because Levat founds several important conclusions on them—viz., that these deposits are the result of the hydro-thermal decomposition of the nickeliferous serpentine, by the agency of numerous iron and manganiferous springs having found vent through fissures, eating the rock away, and leaving half-dissolved masses of it in the earthy deposits, which are found crowned with oolitic iron-ore, the product of these overflows of ferruginous water. Levat concludes from this that the nickel-ore will be found to extend no deeper than the basins of red earth, derived from the decomposition of large surface areas of serpentine; and he states that these follow a certain number of N.E. and S.W. zones, seldom exceeding 600 yards in width, which can be traced from the eastern coast towards the west of the island. It seems to me that just as the nickel-ore is found penetrating the walls of these deposits, so it might be found below "the basins," as deep as the joints extend through which the water found exit. There is frequently, it may be remarked, a close connexion between nickel and serpentine, and as the Sudbury veins of nickleiferous pyrrhotite were originally found carrying copper at surface, and were, indeed, actually opened up as copper mines, it seems possible that the copper veins Mr. Cox speaks of in the serpentines of New Zealand might, if proved in depth, carry nickel. The fact that serpertine is derived from the decomposition of peridote rocks, which are generally admitted to be of igneous origin, would, I venture to think, offer a satisfactory explanation of the nickel deposits of New Caledonia being found at high elevations, as the original rock-masses from which the nickeliferous-serpentine was derived, were no doubt the latest and deepest seated volcanic overflows, which one would expect to find at a high altitude, as if they extended to the low-lands they would stand a great chance of being denuded off the flanks of the hills. I do not believe in working "prospects" on too advanced and expensive lines—in fact, the method of running the stockworks by quarrying and tunnelling seems to me

essentially practical, though in some respects primitive. I refer, for example, to the transport, and think that English engineers might give the Frenchmen a few points in their methods. Whilst I fully approve of the single-rope system, because it is often convenient to adopt where you have to frequently shift your terminals, I do not think the carriage of the sacks and suspenders on men's shoulders, even though convicts, is economical, and "labour" is the most precious item a mine-manager is called upon to avoid expending unnecessarily. If mechanical power is not available, a hand-windlass might be rigged up to haul the sacks back on a carriage, swung from the main-cable, using a small pulling-rope. I am given to understand, on good authority, that the output of these mines in 1891 was 35,000 tons of 10 per cent. nickel ore, and I would therefore like to know if the output which Mr. Garland puts at 60,000 tons at present may not be the crude ore as delivered from the mine. As I understand that 22,690 tons of nickel ore were exported in 1890, the increase in the preceding twelve months was less than 13,000 tons, and it seems a large order, to jump in 1892 to 60,000 tons.

The CHAIRMAN: Mr. Garland in his paper refers to the east and west parts of the island. I notice in an article in the *Mining Standard*, of Sydney, published in 1892, that a statement is made to the effect that the western portion of the island contains richer mines than the eastern, which is

nickel ore rather appear to corroborate that. I think we are all agreed as regards the term "chocolite," that it is unnecessary to introduce further names. It appears really to be garnierite mixed with limonite. I gather from Mr. Claudet's analysis that the water of hydration is given off at two different temperatures, and it would be interesting to know whether the water given off at one temperature is due to limonite and the other to the garnierite. The analyses on the blackboard puzzle me; they seem to me to be very high. I do not quite understand what the analyses are, because we know what the average amount of nickel is, and also the highest and lowest. The highest would hardly be as high as that shown, I think. There are very important points commercially between the New Caledonia nickel ore and the Sudbury ore depending on the totally different nature of the two ores. In the case of the New Caledonia ore, we have rich ore as it is shipped; but we must not lose sight of the fact that that ore has to be selected from a very large mass of useless material, whereas in Sudbury the sulphides occur in very large masses, which can be quarried. It is nothing more than quarrying rather than mining, and although on the average they do not contain more than about 2½ per cent. of nickel, they are easily accessible, comparatively speaking, and can be dealt with in very large masses, whereas in New Caledonia the ore is disseminated through a very large mass of rock, as Mr. Garland has told us. In many cases you have very thin veins, and at most not very large deposits, so that one has to take that into consideration. As against that, we have the very cheap freight from New Caledonia, and the fact that the ore is very pure, the nickel being free from copper and arsenic. The question of the density of this mineral struck me as one worthy of some little attention. The specific gravity given by Mr. Garland is 3. Now, the mineral itself, as was pointed out by some gentleman during the discussion, is very light, and if you take its specific gravity as it is without removing the air, you will find that it only runs out to about 1½. It is very porous, in other words, and that might possibly affect the question of

dressing. The mineral when crushed up would have a specific gravity of 3, whereas in the mass it has only about 1½, it is so exceedingly porous. Mr. Rickard has told us of certain new methods for dealing with the New Caledonia ore. I did not quite gather where those methods were being carried out, because till quite recently, at any rate, it has been the practice to convert all the New Caledonia ore into matte at the works of La Société le Nickel in Glasgow. Some years ago, when the ore was first discovered, they attempted to smelt it in blast furnaces on the spot and send the metal over here in the form of pig nickel, but it was found that the difficulty of converting that pig nickel into nickel—refining it, in other words—was so expensive that it did not pay to do it. As the result the method of converting the metal into matte was pursued, and has been carried on until, at any rate, quite recently, and, as far as I know, is still being pursued. It is perfectly true that at that time there was no use for ferronickel, the use of nickel in the manufacture of steel had at that time no existence. It is now largely used in steel, and that might to a certain extent modify the condition of things. Mr. Rickard, however, does not tell us that ferronickel is being produced as the ultimate product, but rather that it is being produced with the object of refining it. Then Mr. Kitto told us something about Mr. Mond's process. Although that process possibly

THIRD ANNUAL GENERAL MEETING.

THE THIRD ANNUAL GENERAL MEETING was held at the Museum of Practical Geology, Jermyn Street, London, S.W., on the evening of Wednesday, March 21st 1894, at 8 o'clock.

Mr. GEORGE SEYMOUR (President) occupied the chair.

The minutes of the last Annual General Meeting having been read and confirmed, the Secretary then read the Report of the Council for the Session 1893-94 :—

REPORT OF THE COUNCIL—SESSION 1893-4.

THE Council have much pleasure in presenting to the Members of the Institution of Mining and Metallurgy their Second Annual Report, being for the year ending February 28th 1894.

The Council congratulate the Members on having finally secured a permanent office, and headquarters of their own, in the City of London, which was occupied on the 3rd March 1894. After much careful consideration and discussion, it was decided that the business of Mining Engineers being almost wholly in the City it would be most convenient to the Members to have the offices there, and, accordingly, they have been located in a most central position in Broad Street House, New Broad Street. It is expected that Members who are home from abroad for a short period from their professional occupations will find these offices a great convenience, as they can have their correspondence directed there, and use the offices for the purpose of writing letters, and as a rendezvous.

The Institution now consists of—

Members	...	...	143
Associates	...	...	66
Students	...	...	38
Total	...	...	247

These figures show that the Institution is in a sound and satisfactory condition, considering the short time it has been in existence. The qualifications of Candidates and the admission of new Members has been subjected to close scrutiny; it being the aim of the Council that a Diploma of the Institution shall be, as far as possible, evidence of the professional training and experience of the Member.

The following Papers have been read at the Ordinary Meetings of the past Session :—

1. "The Mining District of Oporto," by Mr. Frank Merricks (Associate).
2. "Gold Mining in India," by Mr. A. Mervyn Smith (Member).
3. "Mining in Peru," by Mr. Arthur L. Pearse (Member).
4. "The Electro-Cyanide Method of Gold Extraction," by Mr. J. B. Hannay (Member).
5. "The Economic Treatment of Low Grade Copper Ores," by Mr. J. H. Collins (Vice-President).
6. "The 'Direct' Method of Producing Refined Copper,"

have under consideration the question of abstracting the more important Papers published in other journals at home and abroad.

The Council have in view the establishment of a Library, in connexion with the Institution, of works connected with Mining and Metallurgy. A beginning will be made by exchanges of Proceedings with other similar institutions in various parts of the world, and it is hoped that Members of the Institution will make donations to the Library.

The Hon. Treasurer's Statement of Accounts and Balance Sheet for the first two years shows that the income of the Institution is ample for all present requirements, and, as it cannot fail to grow from year to year, a gradual accumulation of funds should take place, which will lend stability to the Institution, and aid in its general usefulness.

The Council announce with much regret that they have lost three members by death during the year just ended: Mr. H. M. Becher, to whom the Institution was indebted for the first paper; Mr. A. M. Levy, and Mr. George Simpson.

The Council trusts that the Members of the Institution will contribute Papers for reading and publication in the Proceedings, this part of the work of the Institution being that which is of the greatest permanent importance. If Members of the Institution, when engaged on the examination of mining properties in all parts of the world, would make notes on the mineral deposits and other matters with a view of communicating them to the Institution, there is no doubt that a mass of extremely valuable knowledge would be accumulated on what is at present but a little known science—the theory of the formation of mineral veins and deposits.

GEORGE SEYMOUR, *President.*

S. HERBERT COX
PERCIVAL FOWLER
C. ALGERNON MOREING } *Hon. Secretaries.*

The CHAIRMAN: Gentlemen—Our current session having ended, and a change in the officers of the Council having been caused by the effluxion of time, the following appointments have been made: Professor Huntington becomes the new President of the Institution, and the Vice-Presidents are Mr. J. H. Collins and Mr. Joseph Garland. The members of the Council, as at present constituted, are, besides the three gentlemen just named: Mr. Charles J. Alford, Mr. George A. Ferguson, Mr. Benedict Kitto, Mr. Walter McDermott, Mr. James Mactear, Mr. Edward Riley, Mr. Edgar Taylor, Mr. William Thomas, and Mr. Claude Vautin; the Honorary Treasurer is Mr. Arthur C. Claudet; the Honorary Secretaries are Mr. S. Herbert Cox, Mr. Percival Fowler, and Mr. C. Algernon Moreing; while I myself, as retiring President, become an *ex officio* member of the Council. I may perhaps point out, for the information of those who are not aware of the change in the honorary secretaryship, that Mr. George A. Ferguson, having resigned, it was deemed expedient by the Council of the Institution to have three honorary secretaries, for the simple reason that, owing to the precarious mode in which mining engineers have to gain their living, it is quite possible that one or two of them might be absent at the same time. As regards the accounts, I may mention that, owing to the unfortunate illness of our late Honorary Secretary, which has, unhappily, proved of rather long dura-

trust, however, that they have been successfully overcome, and, having acted in some degree as nurse to the Institution since its commencement, I trust that I now leave in your hands, on retiring from the chair, the control of a robust and sturdy infant. Our thanks are due to Mr. Geo. Ferguson, our late Honorary Secretary, for the great time, trouble, and labour which he devoted to its success; and also to the proprietors of the *Mining Journal*, for the hospitality they afforded to us during the earlier periods of our career. In introducing my successor to the chair, I think it a happy idea that one so well versed in metallurgical science as Professor Huntington should take the Presidency of this Institution, and I think it is also desirable that in future, in accordance with the name and spirit of the Institution, the President should be selected alternately from the branches of mining and metallurgy. Again thanking you for your kindness in the past, and wishing you all and every success, I beg to introduce to you my successor, Professor Huntington.

Mr. Seymour then vacated the chair in favour of the new President.

PRESIDENTIAL ADDRESS.

By Prof. A. K. Huntington *(President)*.

Gentlemen — No president worthy of the name and its traditions ever begun an address without an excuse and an apology from him for his and its shortcomings. Doubtless mindful of this, and wishing to do me special honour by affording me an opportunity of "maximising" the excuse, and thereby minimising the apology necessary, the Council kept me in the dark until within a month of now of its intention to elect me President of this Institution. Whilst I am obliged to the Council for the honour it has done me, on the whole, I should have preferred to have had a little less opportunity for excuses, and a little more time in which to collect ideas and suitable materials upon which to base my remarks to you. For, although I have thus far spoken lightly, I feel deeply the great responsibility I have undertaken. The thought that I should be President for this year had never entered my mind. I had looked upon it merely as a remote possibility of the dim future, which might safely be ignored for the present. We are all slow to learn that we are, to a great extent, but the

Is the Institution Necessary?

I may, perhaps, be allowed to go one step further back, and put the question: Is this Institution necessary? For it is possible to have potential energy which is not required where it is found. Such is the case, unfortunately, in this small island; the labour market, in fact every calling, is overstocked. Yet for this potential energy there are plenty of outlets possible in our great colonies, if only, in electrician's parlance, "good contact" could be made, and "a connexion" established.

Is, then, this Institution required? That it exists does not necessarily argue that it ought to exist, or is capable of doing good work. If, however, on reviewing our position, we decide in the affirmative, then let us one and all put our shoulders to the wheel, and lift ourselves out of the quagmire of uncertainty surrounding the early steps of every institution into a more elevated position of assured success. I do not for one moment wish you to understand that I consider there is any doubt as to our having already achieved a great measure of success. There can be no doubt about it, but much remains to be done.

The Inauguration of the Iron and Steel Institute.

It may encourage us and guide us if we look round and note what has happened in the case of others who have been somewhat similarly circumstanced to ourselves. Take the Iron and Steel Institute, for instance, which was established in the year 1869, with, up to a certain point, the same objects as we have in view, only limited to those who are engaged in the production and manipulation of iron and steel. The iron and steel industries far surpass all others of the same class in this country; in fact, it is no exaggeration to say that they have mainly contributed to make the British Empire what it is. Yet one year more has to run its course before the Iron and Steel Institute can boast of the small span of existence comprised in a quarter of a century.

The Iron and Steel Institute started under the most

favourable auspices. It had for its first President the late Duke of Devonshire, a man of exceptional ability, who was no mere figurehead; his address was highly technical, and characterised by a clearness and comprehension of the subject which could hardly be surpassed. The late Earl Granville was also one of the first members enrolled, and he took an active interest in the Institution as long as he lived.

The Duke of Devonshire, in his address, remarked that "The advantages of mutual co-operation among those engaged in a common pursuit, whether scientific or practical in its character, are so manifest that to enlarge upon a proposition, the truth of which is universally admitted, would be a needless waste of your time." And further on he said: "To anyone, indeed, who possesses a general knowledge of the enormous scale on while this manufacture is now carried on, and who is, at the same time, aware how important is the aid which chemical science has already shown itself able to render both towards a true understanding, and towards the improvement of the processes by which the raw ores are converted, and how essential are the services of mechanical science in the various applications of the manufactured products, it must be a matter of surprise that an institution of this kind has not been long ago called into existence." The President made no attempt at explanation, but he added the significant remark that: "The

"had become our superiors in practical and scientific skill.' This statement was subsequently refuted by certain well qualified persons, who made a careful examination of the continental works. Amongst them was Mr., now Sir, Lowthian Bell, who reported that "If the distance which separates us as ironmasters from foreign nations has diminished in recent years, this circumstance is rather due to the vigorous manner in which we have been followed, than from any want on our own part in leading the way." Looking back we see that these words very accurately described the position of affairs.

The South Wales Institute.

The scare they had had made those concerned in the iron industries realise fully that they must strain every nerve in order to prevent the extent of their lead being seriously diminished. The iron and steel industries of this country had jog-trotted along for several centuries an easy first, and it is much to their credit that they realised even so late as they did the advantages which would result from co-operation on a national scale. In this connexion it is interesting to note that local co-operation in the industries in question had been initiated many years before. And what was the incentive? Why, the struggle for existence; the realising that to survive they must accommodate themselves to altered circumstances. Then competition was not between this country and foreign nations, but between one part of this country and another. Thus, in 1857, the South Wales Institute of Engineers was formed, twenty-two years before the Iron and Steel Institute. The first president was Mr. Menelaus, the manager of the great Dowlais Iron Works—a man whose name is probably little known to the rising generation, but who contributed largely to make the iron and steel industries of this country what they are. The chief reason Mr. Menelaus gave why they should co-operate was that they were losing ground from the exhaustion of their cheaply worked minerals, and that the cost of labour was

increasing, which would necessitate the substitution of machinery. The Institute was formed, he said, to help them to compete with their more favourably placed neighbours. That was thirty-seven years ago, and the local ores have since practically been exhausted; but not the vitality of the undertaking, one of the largest of its kind. Determined to remain at the front, by keeping pace with the requirements of the times, under the able management of Mr. E. P. Martin, they have recently boldly moved the plant from Dowlais, and rebuilt the works on the most modern plan at the port of Cardiff, where a plentiful supply of imported ores can be obtained, and the expense of railway carriage, always very great, be got rid of.

Thus we see that it was stress of circumstances which originated both the South Wales Institute and the Iron and Steel Institute: in the one case local, in the other national. The progress in the iron and steel industries in the last twenty-five years has been extraordinary, and I have no hesitation in saying that a large share of it is due to the existence of the Iron and Steel Institute. The members of those industries are no longer working against one another in a very narrow spirit, being afraid to confide their ideas one to another lest they should give a competitor an advantage. All this narrowness has disappeared, and now members visit one another's works and discuss everything concerning them at every opportunity. The interests

country has occupied in the trade of the world. Others struggled for the mere crumbs, and it was some time before we realised what a large share of the whole loaf these crumbs might come to represent. This inertia has pervaded the whole industrial policy of our country. The Society of Chemical industry only came into existence in 1881. The Continent had long had numbers of technical journals. We had next to nothing, and it often happened that important information about an English invention first reached us in the form of an abstract from some foreign journal.

Professor Sir Henry Roscoe, in the first presidential address, said: "We have heard much lately about foreign competition, or even the decay of English manufactures. That foreign competition should become year by year keener, and that we who have led the way, and had the advantage of the start in the race, should feel this competition can in no way surprise us. That English manufacturing skill is on the wane, or that the inventive genius of the people is dying out, I, for one, do not believe. Only let the Englishman know what he really needs, and he will soon take care that he gets it, be that need an increased technical or educated skill, or a wider diffusion of the knowledge of the scientific principles. To assist in this direction, and so enable us more satisfactorily to meet the inevitable increase of foreign competition, is one of the objects for which this society has been founded." To the credit of this society, be it said, once started, they have thoroughly well done their work.

There is something deliciously *naive* in Professor Roscoe's remark that you have "only to let the Englishman know what he really needs, and he will soon take care that he gets it." Exactly; you have "to let him know;" he will not foresee for himself. The only way of "letting him know" is to put your hand in his pocket; but he never realises the actual state of affairs until your hand is, as the saying goes, on his purse strings, then he begins to take steps to protect himself, and by means of the great national wealth pulls through somehow or other, but not unscathed.

England's Colonial Policy.

As having an important bearing on the mining and metallurgical industry, another case of great want of national foresight may be instanced. I refer to our colonial policy—if policy it can be called—for it has consisted of a masterful rather than a "masterly inactivity." Yet the position of our country, now second to none the world has known, must in the near future be determined by its relations to its colonies and dependencies. This is seen by all at home who are not immersed up to the eyes in petty party politics blinding them to all else. Other nations are not only fast tending to supply the greater part of their own requirements, but are even trying to oust us in our own colonies.

The foresight up to the present has all been on the part of the colonies themselves. The building of the Canadian Pacific Railway—take but one instance—will be a lasting memorial of the enterprise and sagacity of their leaders. It is they, again, not we, who are taking active steps to brace the British empire round with the only bonds which can ensure its existence and continuance; direct and cheap communication throughout by rail and steamer, and by telegraph and cable.

The Empire may be likened to the human body, the means of transit corresponding to the arterial system,

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colonies. Perhaps after all, then, it was not the destiny of this country to form the British Empire, but only to occupy the position of the head. But, if I may be permitted to carry the simile a little further, it must not be lost sight of that although the head, through the brains, cannot make the general arrangement and shape of the body different from what they are, yet it may, and does, determine the vigour and rate of growth of the body. It may stunt growth, and even induce premature decay. The relation of the head and body to one another must, in fact, be such that the body may freely and fully develope, otherwise the head itself must decline in power from the want of nourishment which the rest of the body alone can supply to it.

And what, it may be asked, has this to do with us as miners and metallurgists? This: that in this country, mining, for anything except coal, ironstone, and tin ore, has dwindled to such small proportions that it may practically be ignored, and it is mainly in our colonies that the members of this Institution are called upon to practice their profession, for the great industry of coal mining is not intended to come within its scope, and it is on materials obtained from our colonies that those metallurgists who stay at home must practice their art and science.

Facilities of Inter-Communication Necessary.

In considering whether mining will pay or not, the cost of transit is unquestionably the prime factor. It is, therefore, of the utmost importance to the members of this Institution that facilities for inter-communication in and with the colonies should be increased and cheapened in every way possible, and with as little delay as may be.

A country may contain great mineral wealth, the indications of which are well defined, and the localities known to many an explorer, and parts of it may be in the highest degree suitable for agricultural pursuits, yet is such a country as useless as the unirrigated desert of Sahara now is until it be brought into touch with civilisation by

railways and telegraph, the making of the material for which alone supplies with the necessaries of life hundreds of thousands of those who otherwise would be starving men and their families here at home.

All honour, then, to such men as Cecil Rhodes, who not merely realises the requirements of the times we live in, but boldly meets them, whilst our politicians at home spend the greater part of their vital energies in wrangling over the special form of "Home Rule for Ireland," unmindful that Ireland is in point of mineral wealth and suitability for agriculture and horticulture vastly inferior to our possessions across the great seas, now, thanks to the progress due to the practical applications of science, but a few days separated from us. Of these vast countries, with their rapidly increasing populations and enormous potentialities they take but little, if any, note. In speaking thus I mean no disrespect to Irishmen, who certainly are not responsible for the geological structure of their country, and who on the field of battle have done this nation such splendid service. It is strange that even in our common language we cannot be reconciled with Ireland. We have no word to express such unity. When we would speak of English, Scotch, and Irish collectively as one nation, we have no word at our command. It we say Englishmen, the Scotch not unreasonably object that their great share in the affairs of the Empire is not recognised by this term.

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tives. Result: 17,000 miles of railway in a vast country containing 250,000,000 inhabitants. The United States of America, with about one fourth of this population, has ten times the extent of railways. What will such a state of things end in? Who can say? It is true some money has been spent on irrigation, but that spent with the least grudge has been on defence against attack from without. May not the attack ultimately come from within? Who shall say? 250,000,000 people, slowly breaking with their old traditions and passing to a new condition of things under the ever-watchful eye of the agitator, ready to exploit them and mould them to his ends at every turn.

In Imperial matters do we not misjudge the relative importance of things? We seem to rate their importance by their nearness to this country in actual mileage, forgetful that science is annihilating distance. Our policy has hitherto been Empirical not Imperial. Let us hope that the dawn of greater things is heralded by two of the greatest figures on our political horizon—Lord Salisbury and Lord Rosebery.

The Imperial Institute.

Another hopeful sign of the germination of a broader Imperial policy is the formation of the Imperial Institute, due to the Prince of Wales, who has ever shown himself active and farseeing where the good of this country and the Empire is concerned. The difficulties attending the organisation of such an institution are very great. Let us hope that the well-known powers of its Secretary, Sir Frederick Abel, himself an accomplished metallurgist, and the assistance rendered by so many of the leading men of the day, will give such results as were foreshadowed at its inauguration. As a sign of the times it may be noted that the social side has not been neglected in the plans of the Imperial Institute. It is, indeed, only by bringing men together that they come to know, understand, and appreciate one another. Asperities and ill-feeling are softened down, and harmony and good feeling result, which are more important factors in the progress of a community

than all the legislative measures ever devised. And this is one of the prime objects of our own institution: Goodfellowship. If only we can accomplish this, much will follow.

We have seen that the iron and steel industries have their Institute, and the coal industry has many scattered throughout this country; but now, with great advantage to all, united for the purpose of publication under the title of the Federated Institution of Mining Engineers.

Perhaps, inspired by the example of an older and greater institution, which has, on more than one occasion, been oblivious of the fact that it has already well-defined and important duties to perform, the Federated Institute of Mining Engineers has overstepped the bounds of its legitimate sphere—all that appertains to coal and coal mining—to endeavour to attract to itself papers which come within the special province of the Institution of Mining and Metallurgy. I would respectfully submit that there is ample work for all, and that the objects which naturally centre in each institution would be better served by each confining its attention to its own.

The Institution of Mining and Metallurgy.

Mining engineers and metallurgists in this country, other than those dealing with coal and iron, had not, until the Institution of Mining and Metallurgy was established, a central institution to promote their interests;

to-day, when we find that the Iron and Steel Institute numbered amongst its earliest adherents most of the well known manufacturers; in fact, within a short time of its starting, I question if there were any of importance not on its rolls. Yet, I doubt if amongst our own members we have to-day one single manufacturing metallurgist. They have remained untouched till now by that vitalising and invigorating influence—severe competition. In fancied security they slumber at the wheel, whilst the ship draws slowly but surely to the rocks.

The ores of copper, lead, silver, zinc, bismuth, antimony, nickel, and cobalt are practically nearly non-existent in this country. They are imported from all parts of the world, often as ballast, when there is no other return cargo.

Those who mine these ores are much at the mercy of the buyers, who, being few in number, combine together and regulate the market for their own ends. The "ring" which encompasses their interests is cunningly devised, and there is little fear of its being broken through from within or without by any one at home, but a rude awakening is preparing for those who continue to slumber.

The American Institute of Mining Engineers.

Outside the charmed circle a great power has arisen and developed with the rapid and elastic step of vigorous youth and this power—I refer to the United States of America, which possesses within its bounds extraordinary mineral wealth—is destined to eclipse the Old World in matters metallurgical. In America the mining engineers were some of the first to join together under the title of the American Institute of Mining Engineers for mutual assistance and support. They knew they had all the world to fight, and they went the right way about it, spurred on by competition rather than deterred. Their Institution came into existence 22 years ago—only two years after our Iron and Steel Institute, before which, be it noted, they had formed an Iron and Steel Association of their own.

The American Institute contains manufacturers, engineers, metallurgists, all working to a common end. They hold meetings in different parts of America, they have met in Canada, and a large body of their members recently visited Europe. In fact, they work on the same lines as our Iron and Steel Institute, but include both the work it does and that which we have set ourselves to do. It is a great institution, admirably managed, and the extraordinary progress which America has made in the last twenty years in metallurgy and mining is in no small degree due to its influence and that of the *Engineering & Mining Journal*, which has worked side by side with it. The spirited policy of this latter journal cannot be too greatly praised or its example too soon followed. The new departure it has made in publishing an annual volume, entitled, "The Mineral Industry, its Statistics, Technology, and Trade," in which the mineral statistics for the whole world are accurately given us, within a short time after the close of the year is a condition of things quite unprecedented. Their enterprise in publishing this volume, which also details up to date the progress made in all branches of metallurgy and mining, is worthy of the fullest recognition and encouragement. It is also from the hands of the United States of America that we have recently received books on copper, lead, and iron and steel of the greatest practical value. Mr. H. M. Howe's first volume on "The Manufacture of Steel"

when, with the development of their shipping, marked signs of which are apparent, we shall find ourselves engaged in a hard struggle for the carrying trade of the world, which has so long been ours that we have grown to look upon it as our birthright, which it would be sacrilege to rob us of. Then will much of the mineral wealth which has hitherto gravitated to us go to feed the furnaces of the rapidly growing establishments of the United States.

Yet these things need not come to pass if we would but hasten to consolidate the Empire, and work together with one aim and object—to remain the Greatest Power on Earth.

Notwithstanding that the City of London, the financial capital of the world, has supplied the means for most great mineral undertakings, there has been no adequate co-operation amongst those concerned to protect and control their interests. Such duties might be expected to devolve upon the London Chamber of Commerce. That they are not unaware of their responsibilities is evidenced by the fact that they have a section for mining, but this section exists only in name—a paralysed and lifeless limb, much requiring to be galvanised into activity. Will it continue to sleep the sleep akin to death till the interests it was created to serve suffer irretrievable decay?

Their great mineral wealth and the compactness of the country give the United States a considerable advantage over us in the education of metallurgical and mining engineers. Everything is in touch there, and money is freely spent on education. In this, as in other matters I have referred to, they are fast leaving us behind, and we must expect to find their engineers competing with our own in fields which should be occupied by ours alone. Are not the most important nickel mines at Sudbury, Canada, owned and worked by Americans, and many iron mines, too? And may not a great mining financial centre arise in New York in competition with our own?

All these things may come to pass, and more; yet it need not be, if we will but arouse ourselves before it is too

late. In administration, our watchword must be organise, organise, organise; and in education, endow, endow, endow. In the same way that the Institution of Civil Engineers has exerted a great and beneficial influence on those following that profession; so should the Institution of Mining and Metallurgy in its own special sphere of action do incalculable good. It is my earnest hope and belief that the Institution of Mining and Metallurgy will in the coming struggle give to our profession that strength and support which alone results from a firm union of hearts and minds.

It is given to few to speak with the polished diction of our late President, whose address has been happily described as "luminous." If light there be in my address to you to-night, it is rather the phosphorescent glow which sometimes results from the stirring up of stagnant waters. May the rising tide of public opinion sweep all stagnation before it, and the prosperity of the British Empire, with which our interests are co-extensive, flow on unimpeded in ever increasing volume down through all time.

Mr. Claude Vautin: I think it very seldom falls to the lot of members of any institution to listen to such an exhaustive, comprehensive, instructive and clever address as the one which we have just heard. We have learned

institution as this, especially in mining; and, as most mining engineers and metallurgists are constantly called abroad, such a consolidation of our colonial interests is necessary to the success of the profession. In one respect I thought Professor Huntington might have enlarged on the sphere of usefulness of this institution. My life has been mostly spent in the service of inorganic chemistry, and when one has conducted a research in this domain, one may describe the actual scientific results to the Chemical Society, but hitherto there has been no society to which the practical results could be communicated, and before the foundation of this Institution there has been no audience before which one could go into such details. The Society of Arts might take a certain interest in these things, but practically they are too broad for a subject like metallurgical mining, so that in this respect I think this Institution is an extremely useful one. What was badly wanted was a body of men to whom one could go and lay before a thoroughly critical audience any application of the scientific principles which one has perhaps already explained to chemists. I second with the greatest pleasure the vote of thanks to Professor Huntington for his address this evening.

The motion was then carried by acclamation.

The PRESIDENT: I thank you very much, gentlemen, for the kindly way in which you have received my address.

Mr. PERCIVAL FOWLER then proposed a vote of thanks to the retiring President for his services, which was seconded by Mr. J. H. Collins, and cordially adopted.

Mr. SEYMOUR, in reply, said: I am very much obliged to you for the kind way in which you have spoken of me. It is true that for a good many years I devoted a great deal of attention to this Institution, which at one period almost threatened to be received as a foundling, owing to the want of legitimate parents. I am happy to say that any such stigma on its character has long since been removed, and I feel sure that under the vigorous guidance of your able President and the other members of the Council it will continue to flourish, and prove a great and lasting success.

FIFTH ORDINARY MEETING.

THE FIFTH ORDINARY MEETING of the Third Session was held on Wednesday evening, April 18, 1894, in the Lecture Theatre of the Geological Museum, London, S.W.

Professor A. K. HUNTINGTON (President) occupied the Chair.

The SECRETARY having read the minutes of the last meeting, which were duly confirmed,

The CHAIRMAN said he was sorry to announce that Mr. Garland was too unwell to attend the meeting in order to reply to the discussion on his paper on "Nickel Mining in New Caledonia." He would therefore call upon Mr. Hannay to read a paper on "The Metallurgy of Lead."

[redacted]

ON THE METALLURGY OF LEAD.

By J. B. Hannay (*Member*).

Having been engaged during the last seven years in the study of the treatment of lead ores, it became apparent very early in my work that the accepted explanations of the chemistry of lead smelting did not accord with the observed facts: so a systematic study of the matter was entered upon, and notes gradually accumulated, which have at length formed a complete explanation of the phenomena observed; and I now venture to bring these results under the notice of the members of the Institute of Mining and Metallurgy. In order that the multitudinous facts may be classified, I have divided the work into the following sections:—

1. The Relations of Lead and Sulphur.
2. Specific Gravity of Sulphide of Lead.
3. Methods of Analyses.
4. Furnace Re-actions of Lead Compounds.
5. Lead Smelting.
6. Proposed New Metallurgy of Lead.

As Percy gave very minute attention to this subject, and epitomised our knowledge of the Metallurgy of Lead at the time of the publication of his work, and as very little seems to have been done subsequently, I take his volume on lead (1870 edition) as a guide in discussing the theories then held.

The present memoir is an attempt to elucidate this subject by an experimental investigation of the various re-actions; and the discovery of some interesting compounds has enabled me to throw some light on the furnace re-actions of lead.

As the smelting of lead sulphide is attained through the burning out of a portion of the sulphur, it was necessary, in the first place, to examine the "supposed sub-sulphides of lead," as Percy calls them. It was found that when any compound of sulphur and lead with less sulphur than the mono-sulphide was fused, it was impossible to pour two samples of identical composition. When the melted "sub-sulphide" was allowed to cool slowly, and samples poured at intervals, there was great variation in the amount of sulphur—those samples poured at the lowest temperatures containing the smallest proportion. It then became clear, that the reticence and indefiniteness of the standard works on the subject of the lower sulphides of lead arose from the fact that no such sub-sulphides exist; but that all sulphur compounds containing less than the theoretical amount of sulphur required to form PbS, are mixtures of the mono-sulphide and metallic lead.

As all fireclay and plumbago crucibles absorb the melted galena and its products of oxidation, the final experiments quoted in the following pages were conducted in porcelain crucibles, placed as a lining inside thin "salamander" plumbago crucibles.

After many experiments it was found that any compound of lead and sulphur may be fractionated (by allowing a portion to crystallise, and pouring off the still fluid

a compound of 7·17 per cent., was melted, allowed to partially crystallise, and parted as equally as could be judged, and yielded:

253 grms. containing 8·29 per cent. of sulphur
and 240 „ „ 5·79 „ „ „

Higher portion 253 grms. yielded:	Lower portion 240 grms. yielded:
162 containing 7·14 p. cent. sulph.	135 containing 6·84 p. cent. sulph.
87 „ 9·62 „ „	98 „ 4·20 „ „
Higher portion 87 grms. yielded:	Lower portion 98 grms. yielded:
50 containg. 8·21 p. cent. sulph.	55 containing 5·27 p. cent. sulph.
35 „ 10·31 „ „	39 „ 1·84 „ „
Higher portion 35 grms. yielded:	Lower portion 39 grms. yielded:
25 containg. 9·36 p. cent. sulph.	8 containing 8·10 p. cent. sulph.
7 „ 12·27 „ „	28 „ 0·00 „ „
Highest, 12·27 per cent. of sulphur.	Lowest, metallic lead.

From the above we see that it is possible to fractionate a compound of lead containing less sulphur than the mono-sulphide into its two ultimate products, lead and its mono-sulphide, because, although the highest proportion of sulphur obtained was 12·27 per cent., it is impossible to melt mono-sulphide (except under extreme precautions, which in this preliminary stage of the enquiry were not used) without volatilising or burning off about one per cent. of the sulphur, so that the above 12·27 per cent. compound really represents the mono-sulphide. It is apparent from this experiment that in the liquid state the compounds Pb_3S and Pb_4S have no existence, but it remained to determine whether or not all compounds of lead containing less sulphur than the mono-sulphide are mixtures of that body with metallic lead, or whether there exists an infinite series of sulphides of lead—the metal arriving at saturation on the proportions of the mono-sulphide being reached. An interesting experiment showing the above fractionation, but conducted by a single experiment, may be performed as follows: A mixture with about 5 or 6 per cent. of sulphur is melted at a bright red heat, removed from the furnace, but kept from cooling too quickly, while the contents of the crucible are made to assume a rapid centrifugal motion by a movement of the arm. By this means the sulphide, as it

comes ripe for crystallisation, is caused to adhere to the sides of the crucible, and the lower sulphide compounds remain liquid. If the centrifugal motion be kept up sufficiently violently, high sulphides are separated out as soon as formed, and the central liquid portion is thus gradually robbed of its sulphur, and finally metallic lead is left. For instance, 100 grms. of a 5·5 per cent. compound yielded in this way 50 grms. of hard lead containing only about 0·05 per cent. of sulphur. This was re-melted and yielded by a similar centrifugal treatment 20 grms. of pure soft lead, leaving 30 grms. of hard lead and dross. This, again treated, yielded 17·5 grms. of soft lead and 12·5 of hard lead and dross which was not further dealt with. 100 grms. of a mixture containing 8·7 per cent. of sulphur when fractionated with as violent centrifugal motion as the hand could give yielded only a few drops of hard lead weighing 8·5 grms. containing 0·88 per cent. of sulphur.

So rapid is this separation into higher and lower sulphur compounds, that it is impossible to pour a portion into the hemispherical cavity of an assay mould without separations taking place of such an extensive nature as to render any analysis of the sample utterly untrustworthy, unless the whole sample is pulverised and intimately mixed before analysis. Below is a diagram representing a section of the button of an 8 per cent. compound poured into an assay mould.

2. Specific Gravity of Lead Sulphide.

When galena is melted in air, a portion of the sulphur is burnt off, but it was found quite easy to prevent this by melting the galena with some sulphur, while covered over with charcoal.

Artificial galena was found to be identical with the natural mineral, but the specific gravity of both varieties varied in an unaccountable manner until it was discovered that it is impossible to bring pulverised galena into contact with water, much less to boil it (as in specific gravity determinations), without oxidation taking place. The oxidation lowers the specific gravity. When the determinations were conducted in benzine all irregularities disappeared, and the specific gravity of pure galena, whether natural or artificial, was found to be 7.766. Mere warming of the pulverised galena in the water bath to drive off benzine, caused oxidation sufficient to lower the specific gravity by 0.05 each time.

In order to determine, by specific gravity determinations, whether or not any combinations (however weak) existed between the PbS and the Pb in the lower sulphurised mixtures, a series of hypothetical compounds Pb_4S_3, Pb_3S_2, Pb_2S, Pb_3S, and Pb_4S, were prepared by melting the necessary amount of pure lead and galena, then violently agitating and pouring the mixture into a cold steel mortar. Although so suddenly chilled, it was found that considerable separation had taken place, the layers in the crucible varying in composition. The entire samples were reduced to powder, and their specific gravities determined in benzine.

I have tabulated the figures obtained along with calculated results. In calculating these specific gravities, Deville's value for lead (11.254) was used :—

	Per cent. Sulphur.			Specific Gravity.		
	Obtained.	Calculated.		Obtained in Benzine.	Obtained in Water.	Calculated.
PbS	13.40	13.415	...	7.766	7.585	—
Pb_4S_3	10.32	10.41	...	8.51	8.19	8.547
Pb_3S_2	9.29	9.362	...	8.79	8.57	8.820

	Per cent. Sulphur.			Specific Gravity.		
	Obtained.	Calculated.		Obtained in Benzine.	Obtained in Water.	Calculated.
Pb_2S	7.18	7.191	...	9.36	9.09	9.384
Pb_3S	4.89	4.911	...	9.93	9.74	9.977
Pb_4S	3.71	3.729	...	10.25	9.97	10.284

The numbers representing the specific gravities, as obtained in water, are the means of a large number of determinations, as I had repeated the operation again and again, owing to the discordant results, before I tried the Benzine method. I then obtained constant results. These results show that the supposed subsulphides are simply mixtures of the monosulphide with metallic lead.

3. Methods of Analysis.

An examination of the methods of analysis was made in order to find an accurate and speedy method of estimating both the lead and sulphur in the same weighed portion.

A method of oxidation was sought which would speedily yield a pure sulphate of lead from the sulphide, and the following was found to be successful, and to prevent the separation of sulphur.

Strong nitric acid is gently warmed in a beaker, and a few crystals of $KClO_3$ added, and the whole covered with a glass plate. When the liquid has become dark yellow by the decomposition of the chlorate, the pulverised galena is

always contained lead, from which it was impossible to free it, while some of the barium sulphate was retained in solution by the lead salt, and so passed into the filtrate. After a few hours it gradually precipitated along with some lead.

It was also found that when it was attempted to convert the sulphate of lead into carbonate by boiling with a solution of alkaline carbonate, a portion of the lead was soluble in the alkaline solution, and so passed into the filtrate along with the sulphates of the alkali metals, so that in neither case was there complete separation of the lead and sulphur. The following process was finally devised, and was found to yield reliable results for determining both lead and sulphur in the same sample.

The galena or subsulphide of lead was oxidised by the chlorate mixture already described, using 10 c.c. of strong nitric acid to 1 grm. of galena. The mixture evaporated to dryness, and 2 grms. of pure sodium carbonate added with some water. The mixture was boiled for a few minutes to convert the whole of the lead into carbonate and then cooled, 2 grms. of sodium bicarbonate added, and the mixture allowed to digest in the cold for about a quarter of an hour.

It was found by this method that the lead which had become dissolved by the carbonate of sodium was thrown down by the addition of the bicarbonate, so that, on filtering, the whole sulphur passed through as sulphate of soda, while the lead remained behind on the filter as carbonate, and was weighed as sulphate. Care was taken that the amount of water used in this digestion was insufficient to dissolve all the sodium bicarbonate, so that on washing the lead on the filter with cold distilled water the sodium bicarbonate was gradually dissolved away.

By these means the complete separation of sulphur and lead can be effected.

The carbonate of lead on the filter is dissolved in dilute nitric acid and precipitated as sulphate, while the sulphur is estimated as $BaSO_4$ in the ordinary way.

Most of the analyses given in this paper were done by this method. A method that was sometimes used when the

proportions of lead and sulphur approached that of galena was that given by Thorpe in his Handbook of Analyses, which consists in suspending the sulphide of lead, very finely pulverised, in caustic alkali solution, and oxidising it by a stream of chlorine gas. The lead is converted into the di-oxide, and may be heated in a porcelain crucible and weighed as litharge, while the sulphur passes into the filtrate from the lead as sulphate of soda.

This method of separation was found to be very accurate for galena, but as it involved the use of chlorine gas, and was inapplicable to lead containing little sulphur, it was found preferable to make use of the method I have already detailed.

4. Furnace Reactions of Lead Compounds.

We have seen that the text-book information on the sulphides of lead, their specific gravities and analyses was lamentably faulty; but it is when we come to deal with the re-actions of lead compounds at high temperatures that we see how little is known about the chemistry of the furnace reactions of lead.

Percy says that the chemistry of lead smelting is unexplained. He gives in his book various reactions which will now be examined seriatim. For instance, $PbS + PbSO_4 = Pb_2 + 2SO_2$ is given as the whole

in thin salamander crucibles, inside of which were placed porcelain capsules of the requisite size. This was necessary for another reason. The lead compounds adhere so firmly to all crucibles that it is impossible to remove the contents from the crucible, so it was convenient to remove the capsule and weigh it with its contents.

In most cases, the contents were poured into an iron mould so as to be ready for analysis, and the capsule weighed with what adhered to it.

The sulphide of lead used had about 1 per cent. of impurity, so in the first experiment 120 grams of PbS were intimately mixed with 151 grams of $PbSO_4$—*i.e.*, in the proportions of half the mol. wts. This was fused in the porcelain capsule inside a salamander crucible in a Fletcher's gas furnace, and on pouring, it was noticed that instead of lead, there appeared to be about equal quantities of lead and litharge. The litharge was of a dark greyish colour. The residue weighed 188.4 grams instead of 206.4, as it should have done, had the reaction gone as by the above equation.

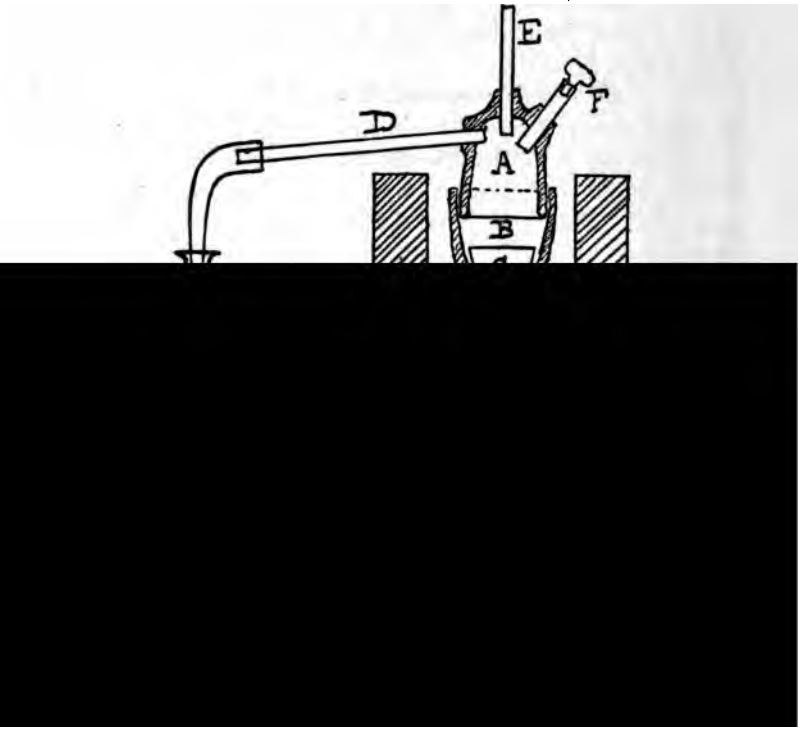
As a considerable amount of fume had been evolved, I thought that perhaps the air had something to do with the non-success; so although the fusion had been done in a well-covered crucible, it was determined to repeat it in nitrogen. This was done by grinding a small plumbago crucible to fit in as a dome to the larger one, and passing a carbon tube through a hole in the top of the dome, and passing a slow current of pure nitrogen. The same proportions were used as before, and this time the residue weighed only 185 grams, showing a loss of nearly 32 per cent., instead of 23.7 per cent. Again it was noticed that a considerable quantity of lead fume was given off. An experiment was done with a smaller quantity, using only one tenth part of the above weights—viz., 12 PbS and 15.1 $PbSO_4$, which gave a residue of 18.7 grams, so that the reaction seemed quite constant and independent of the mass used. The proportions of lead to the dark, coloured litharge were as 86 to 93.

In order to see what was going on, the sulphide of lead

was melted in the porcelain capsule in an atmosphere of nitrogen, and the sulphate added in little pieces and rough powder, momentarily removing the cover for each addition of sulphate.

Twelve grams of sulphide were melted and the sulphate gradually added. It was seen that on each addition of sulphate there was a very violent evolution of sulphurous acid, and a considerable amount of some lead compound accompanied it, and in this case the residue weighed 17·8 grams, so that there seemed to be a greater loss by adding the sulphate gradually than when the two were mixed before fusion. A repetition of the experiment yielded precisely the same results.

It was evident that there was a very considerable volatilisation of some lead compound formed in the reaction and an attempt was made to condense the body in the following manner :—



to the end of D and the point made to dip into caustic soda solution. The joints were made with silicate of soda. Twenty-four grams weight of PbS was placed in the capsule C, and the apparatus made up as air-tight as possible, a slow current of nitrogen passed. The temperature was raised to a very bright red heat, and the $PbSO_4$ to the extent of 30 grams sufficient for the equation $PbS + PbSO_4 = 2\,Pb + 2\,SO_2$ added through the tube F, closing it at once after each addition. Fifty-four grams were used and the residue weighed 36 grams.

It was early seen that the volatile lead compound, most of which condensed in the tube D was black, and on examining the tube when the reaction was complete it was found, when viewed under the microscope, to consist of minute cubes of sulphide of lead. Of this there was collected from the tube and receiver 5·5 grams, and the caustic soda solution, on oxidation by passage of chlorine, yielded $BaSO_4$ equal to 10 grams SO_2.

It was plain from the beautifully regular crystalline form of the PbS, that it had been condensed from a state of vapour, so it became a question of whether the reaction had caused a special volatilization of the sulphide of lead, or whether it simply represented the ordinary volatility of the sulphide of lead.

For this purpose an apparatus was fitted up as at Fig. II.; but the tube E was made to dip to the bottom of C, and the tube F was dispensed with.

A quantity of sulphide of lead was melted in C, and nitrogen passed rapidly, so that the liquid sulphide was splashed about.

Little fume was formed, and the tube D was only slightly stained. By looking down obliquely through the hard-glass tube D, which was only slightly softened by the heat, the molten sulphide could be clearly seen in a state of violent agitation, with little formation of fume. On mixing even as little as 1 per cent. of air with the nitrogen, fumes rose and blackened the tube D, so that it appeared that the volatility was in some way due to oxygen.

The experiment was repeated with hydrogen with the same result, but the fact was noted that if hydrogen was passed from the time the crucible was cold, little fume resulted, whereas when the hydrogen was only applied after the sulphide had been fused, a rush of sublimate came off at first and then ceased.

The rush of sublimate on passing hydrogen after fusion was found to be due to the formation of water by the hydrogen reducing some litharge which had been formed during fusion.

On passing sulphur dioxide copious emission of fumes took place, and the sublimate condensed to a gray powder with a silky lustre, which was due to its being composed of cubes. Steam also gave copious evolution of sulphide, and in this case the crystals were very much smaller. Carbon dioxide also caused the volatilization of the sulphide, as also did carbon monoxide.

The cause of the fume given off by galena when heated in air now became apparent. A portion of the sulphur is burnt to sulphur dioxide, which combines with a molecule of lead sulphide and carries it off. This was tested in the following manner: air was passed slowly through 50 grms. of molten sulphide in the apparatus Fig. II., and the fume which was composed of cubes of PbS was condensed as well as possible, and the SO_2 absorbed by caustic soda.

Air was passed for some time after the emission of fumes

sulphide always condensed in the upper part of the dome and did not pass into the tube D, so that the yield always seems a little low.

The temperature was raised to white heat and the air passed more rapidly when practically the same result was obtained.

Lastly, the experiment was repeated at a white heat in a rapid current of pure oxygen when there resulted 22 grms. of pure soft lead, 4.5 grms. of PbO, and 5.5 grms. of SO_2. The sublimate which had been oxidised to sulphate of lead was not collected.

From these facts it appears that the re-action is independent of temperature, and may be represented thus :— $2\,PbS + O_2 = Pb + PbS_2\,O_2$ ($PbS.SO_2$), and the other gaseous oxides, CO_2, CO, H_2O, seem to form volatile double compounds with PbS.

I may here mention that, reasoning from the behaviour of other compounds formed from a solid and a gas, it was expected that by heating liquid sulphur dioxide with finely pulverised sulphide of lead in sealed tubes under pressure, the volatile compound PbS_2O_2 would be formed. But lead seems always to act in the most unexpected way. On heating sulphide of lead with liquefied sulphur dioxide to a temperature of 130° C, just under the critical point, no action seemed to have taken place; but on cooling down to about 45° C beautiful needles separated out and crystallised, sometimes stretching across the tube about ⅛ in-bore. It was attempted to increase the amount of this substance by prolonged heating, but after 4 hours little more seemed to be formed.

Thinking that the crystals might arise from a trace of moisture in the tube, means were taken to insure perfect freedom from moisture; but still the crystals appeared, and in the same small quantity, the remaining sulphide appearing to be unacted upon. Stronger tubes were filled and the temperature raised to 350° C. It was seen that after an hour's heating the gas in the tube was dark brown, so it was thought that the volatile compound had been formed, but on cooling, yellow drops of sulphur separated, which

finally solidified and the crystals again appeared in the liquid.

The crystals turned out to be prismatic sulphur, and the brown gas sulphur vapour, so that no volatile lead compound had been formed, but the reaction had been $4PbS + SO_2 = 2(PbS\ PbO) + S_2$.

It is characteristic of the unexpected manner in which lead compounds act that the reverse of this action—*i.e.*, $4PbO + S_3 = (PbS\ PbO)_2 + SO_2$, does not take place when sulphur is heated with PbO and SO_2 in a sealed tube. When sulphur is added to molten PbO, the oxysulphide compound is at once formed, the above reaction taking place, yet the PbO is absolutely unchanged by the action of sulphur dissolved in liquid SO_2 even when heated up to 300° C.

The fact that the compound only exists at a high temperature and is decomposed on cooling is so singular, and, in fact, exactly the opposite of what usually occurs with compounds of a solid and a gas, which usually dissociate at high temperatures—a few experiments were performed to learn something of its nature.

The tube D, in Fig. II., was turned slightly up and placed in a tube furnace, and SO_2 passed through molten galena in C. The gas PbS_2O_2 came off freely and passed along through the combustion tubing D. It was seen that when the tube was raised to such a temperature that the hard glass was softened, the gas passed without depositing any

These gases being in contact with the sulphide while in the nascent state take up the full quantity of PbS, and so 2 molecules of H_2SO_4 carry away 5 molecules of PbS.

On passing the Pb S_2O_2 compound over molten PbO the compound is decomposed, and the PbS combines with the PbO, forming the compound (PbS.PbO), setting the SO_2 free, and forming the lead member of this new group of compounds.

This substance PbS.PbO plays an important part in the metallurgy of lead, as we will see subsequently.

Nascent sulphur dioxide acts much more perfectly in forming PbS_2O_2 than free gas passed from an extraneous source; but as the temperature approaches white heat, and, by passing the gas slowly, a result can be obtained agreeing closely with the formula PbS_2O_2—*i.e.*, every molecule of SO_2 combines with one of PbS. But little or no PbS is taken up by free SO_2 when passed over solid PbS at a dull red heat, but when air or oxygen is used, the SO_2 produced by the burning out of sulphur in the PbS being in contact with PbS in its nascent state, produces the compound, and a considerable evolution of fumes is the result. The discovery of this volatile lead compound affords the key to much that was formerly very puzzling as regards the behaviour of lead compounds. It shows how a substance which is practically fixed at a white heat in neutral gases,* may give off copious fumes at a moderately low temperature in air, and it affords a very complete explanation of the extreme difficulty of condensing lead fume.† I have

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* Professor Roberts-Austen has since proved that PbS is very volatile in nitrogen. (*Proc. Chem. Soc.*, No. 141, 1894.)—A. K. H.

† It is very probable that the first action of air is to form a low oxysulphide of lead, which is subsequently split up, as I have lately discovered that it is not pure lead sulphide, but a product containing a minute quantity of oxygen, which passes through the pores of the crucible as mentioned by Percy, and that when the sulphide is melted in an atmosphere of hydrogen, it ceases to pass through. Chloride of lead also illustrates this. When melted and electrolysed in an open vessel, oxygen is evolved from the positive pole instead of chlorine, and yet no detectable quantity of oxychloride has been formed, but the

spoken of the sublimate condensing in beautiful cubes, but this only takes place when concentrated and unmixed with a large volume of furnace gas. On the contrary, when the fumes are allowed to escape direct from the crucible into the hot gases rising from the gas furnace, they rise 4 or 6 feet without showing more than a slight blue haze. Meanwhile, the PbS is becoming partially oxidised, and losing its power of crystallising in the cubic or any other system. When it passes into the air, far enough from the furnace to become cooled, it has become so thoroughly mixed with other gases that it becomes visible as a grey haze of the very finest tenuity, which is the constant characteristic of lead fume, so that flues or tunnels even a mile long do not suffice for its condensation.

Having determined the source of loss in the experiments on page 180, we are now in a position to formulate what occurs when sulphide of lead reacts with sulphate, and also with oxide.

Amongst all the formulæ given in chemical works, and especially collected in Percy's Metallurgy on the furnace reactions of these compounds of lead, there is not one which really expresses the truth. The main cause of this failure to arrive at any true explanation of lead reactions lies in the fact that the existence of the volatile compound was hitherto unknown. This is rather curious, inasmuch as the fuming of lead has been observed and unexplained

is going up the chimney. Percy ultimately says that the chemistry of lead smelting is still unexplained. But even where volatility is not in question the real truth was not known.

The only reaction given by Percy which is practically correct misses out an extremely important point. In acting upon sulphate of lead by metallic lead, the formula $PbSO_4 + Pb = 2PbO + SO_2$ is practically what takes place, yet there is a minor reaction going on simultaneously —viz., $PbSO_4 + 4Pb = 4\,PbO + PbS$, and this formation of sulphide of lead renders the litharge obtained by this reaction of little use in most commercial manufactures.

But to return to what I have named the "Sheet Anchor" of the old theory of smelting. We have seen that $PbSO_4 + PbS = 2Pb + 2SO_2$ does not take place, but that something much more complex occurs.* As a certain portion of the added reagent is being thrown out of action by volatilisation, and as changes in the reaction are caused by the products gradually accumulating in the residue and taking part in the reaction, the addition of $PbSO_4$ to PbS, or *vice versâ*, goes through a series of steps which also vary a good deal with temperature. During the seven years in which I have been engaged on this subject, I have traced all these reactions by hundreds of experiments, and as the results clearly establish the course of the reactions, but are too voluminous to quote *in extenso*, I will explain the reactions and merely quote the experiments, which are required to prove at certain stages what is going on. Some of these experiments have been done on large quantities of several hundred-weights, and others on small quantities in the laboratory; but all have been repeated many times.

When sulphide of lead is melted in large quantity, and a small quantity of sulphate of lead added at a high temperature, the addition being made gradually, the following

* Professor Roberts-Austen has since experimentally confirmed the fact that the equation $PbS + 3PbSO_4 = 4PbO + 4SO_2$ is substantially correct. (*Proc. Chem. Soc.*, No. 141, 1894.)—A. K. H.

reaction takes place. The dark-faced formulæ indicate the volatile portion :—

(1.) $12PbS + PbSO_4 = \mathbf{2PbS_2O_2} + 2Pb + 9PbS$.

It is impossible to obtain this reaction rigidly true, as minute portions of the sulphate react with the lead set free before it has time to be absorbed in the remaining sulphide, and so litharge is produced, which reacts on the sulphide and introduces complications. Were it not for these complications, the reaction would proceed just as by the air oxidation of the sulphide—*i.e.*, one-half of the lead would be driven off as PbS_2O_2, and one-half reduced to the metallic state, thus :—

(2.) $3PbS + PbSO_4 = \mathbf{2PbS_2O_2} + 2Pb$.

So that as PbS_2O_2 has the same molecular weight as $PbSO_4$, there would be exactly double the weight volatilised which was added.

Let us see what actually takes place.

24 grms. of PbS were melted and 2.5 grms. of $PbSO_4$ added, the residue weighed 22.1 grms., and 1 grm. of SO_2 was condensed. By reaction (1) there should have been 21.4 grms. of residue and 1.07 grm. of SO_2 given off.

On analysis of the remaining sulphide, the ratio of lead to sulphur was as 11 to 9, as required by theory. On adding $PbSO_4$ to melted PbS, in molecular proportions, for the reaction on page 173, which is supposed to represent

heat, the reaction gave more of the volatile compound, and assumed the form :—

$$(4.)\ 12PbS + 12PbSO_4 = \mathbf{6PbS_2O_2} + 12Pb + 6PbSO_4 + \mathbf{6SO_2}.$$

If the capsule be at once removed from the furnace when the last of the $PbSO_4$ has been added, the metallic lead nearly equals the amount required by the equation (4); but the Pb reacts on the $PbSO_4$, giving litharge and sulphur dioxide, and the delay in getting the crucible out of the heat usually allows the secondary reaction to proceed to some extent. Thus, in one case, 3 molecules of lead had reacted, and at the moment of pouring it stood :—

$$(5.)\ 12PbS + 12PbSO_4 = \mathbf{6PbS_2O_2} + 9Pb + 6PbO + 3PbSO_4 + \mathbf{9SO_2}.$$

24 grms. PbS and 30 grms. $PbSO_4$ (in all 54 grms.) gave 36 grms. residue containing 20PbO and $PbSO_4$, which on analysis gave 8 grms. of $PbSO_4$, leaving 12 grms. PbO, and there was 16 grms. of lead.

The amounts required by theory would be a residue of 34·1 containing 11·1 grams PbO, 7·5 grams $PbSO_4$, and 15·5 grams Pb. At these high temperatures it is difficult to prevent some oxidation by the air, but these numbers agree very closely with the formula.

Another experiment which was carried on till all evolution of SO_2 ceased, when the reaction by complete interaction of the Pb and $PbSO_4$ gave :—

$$(6.)\ 12PbS + 12PbSO_4 = \mathbf{6PbS_2O_2} + 6Pb + 12PbO + \mathbf{12SO_2};\ \text{or,}$$

$$2\,PbS + 2PbSO_4 = \mathbf{PbS_2O_2} + Pb + 2PbO + 2SO_2.$$

24 grams PbS and 30 grams $PbSO_4$ gave 33·4 residue, containing 10 grams Pb, and the residue consisted of PbO with a little PbS, which always forms in this reaction. Theory requires 32·5 of residue containing 10·3Pb and 22·2PbO.

When we reverse the reaction and act upon molten $PbSO_4$ by increasing quantities of PbS, we obtain a different set of reactions.

On adding 1 molecule of $PbSO_4$ to 12 of melted PbS, we

saw it gave rise to 2 molecules of the volatile PbS_2O_2, or that as $PbSO_4$ and PbS_2O_2 have the same molecular weight, the volatilisation was double the addition.

On adding PbS to melted $PbSO_4$ we get as principal reaction the following :—

(7.) $3PbSO_4 + 5PbS = \mathbf{4PbS_2O_2} + 4PbO$.

So that 5 molecules of PbS yield 4 molecules of PbS_2O_2, but as the molecular weight of the latter is higher, 39·7 of PbS would cause a volatilisation of 40·2 of the PbS_2O_2, or about equal quantities.

This reaction only takes place with large excess of $PbSO_4$, as the PbO accumulating sets up secondary reactions.

20 grams $PbSO_4$ were melted, and 2·3 grams of PbS added little by little. The residue weighed 19·9, showing a loss of 2·4 as against 2·33 by theory.

When a greater quantity of PbS is added the reaction takes this form :—

(8.) $12PbSO_4 + 3PbS = \mathbf{2PbS_2O_2} + 2Pb + 11PbSO_4$.

The 2Pb react with $2PbSO_4$ yielding $4PbO + 9PbSO_4 + 2SO_2$, so the final reaction is :—

(9.) $12PbSO_4 + 3PbS = \mathbf{2PbS_2O_2} + 4PbO + 9PbSO_4 + \mathbf{2SO_2}$.

I have stated the reaction in these stages, as metallic lead is seen to be set free at first.

[redacted]

the sulphate there was a very rapid disengagement of SO_2, and the crucible was rapidly dissolved. It was evident that the $PbSO_4$ was being converted into silicate, which as far as the weighings are concerned is exactly like being decomposed thus:—$PbSO_4 = PbO + SO_2 + O$, because the silica comes from the crucible and is weighed with it. The extent to which this reaction is carried at high temperatures surprised me, for the whole fusion and reaction did not take more than 20 minutes, yet in that short time an experiment with $PbSO_4$ alone showed that nearly two-thirds of the sulphate had become decomposed. By many experiments and analyses the following was arrived at as a type of what goes on.

(10.) $15PbSO_4 + 5PbS = \mathbf{4PbS_2O_2} + 4PbO + 12PbSO_4$, but the $PbSO_4$ has been split up by the silica, $12PbSO_4 = 4PbSO_4 + 8PbO + 8SO_3$. So that the total reaction stands thus:—

(11.) $15PbSO_4 + 5PbS = \mathbf{4PbS_2O_2} + 12PbO + 4PbSO_4 + \mathbf{8SO_3}$.

The above equation requires 12·3 grms. to be volatilised on the quantities above given and 25·7 grms. of a residue. These figures agree with the mean of those obtained. I would here remark that those which showed greater loss of weight usually contained more metallic lead. I have noticed that when pure litharge or carbonate of lead is suddenly thrown into a white hot crucible a large part is often reduced to metallic lead apparently by dissociation, whereas when raised to the same temperature slowly no metallic lead is seen. This greater stability is connected in some way with the formation of minute quantities of silicate of lead. The appearance of metallic lead in greater quantity than can be accounted for by any method of double decomposition known to me, I can only attribute at present to dissociation. I have sometimes been led to believe that at high temperatures a more highly oxidised compound than PbS_2O_2 formed the volatile constituent of the reaction, but I have no satisfactory evidence of its existence. Its existence, however, would account for the greater loss by volatilization without calling in dissociation.

We saw that the addition of $PbSO_4$ to molten PbS resulted in an action equivalent to burning out of the sulphur, leaving a sub-sulphurised lead, and now we see that the reverse treatment—*i.e.*, adding PbS to molten $PbSO_4$ results also in a loss of sulphur and formation of a basic sulphate, or a mixture of PbO and $PbSO_4$.

When large quantities of PbS are added to $PbSO_4$, the reaction becomes complicated by the accumulation of PbO in the sulphate, and when the temperature was kept high the sulphate still continued to react in part, but if the temperature was allowed to fall as the melting point of the mixture became lower, the sulphide ceased to react with the sulphate, and formed combinations with the oxide. To follow what was going on it became necessary to examine the reaction of litharge with galena.

Percy gives the main reaction as—

(12.)... $PbS + 2PbO = Pb_3 + SO_2$.

And to show that this is the case, and that excess of either re-agent does not affect the result, he gives

(13.)... $PbS + 3PbO = Pb_3 + PbO + SO_2$; and

(14.)... $2PbS + 2PbO = Pb_3 + PbS + SO_2$; thus showing that the excess of reagent remains unacted upon.

On mixing finely-powdered PbS and PbO in their molecular proportions, and carefully fusing them in an atmosphere of nitrogen, there was formed a steel gray

with various oxides, of which the sulphur dioxide member plays such an important part in all furnace re-actions of lead, so that instead of PbS_2O_2 or $PbS.SO_2$ we have Pb SO, or PbS.PbO. On heating it to a higher temperature in an atmosphere of nitrogen, a little SO_2 was given off, and the substance in the crucible had settled into two layers, the lower consisting of a quantity of semi-sulphurised lead, and the upper of a black slag.

This was treated with ammonium acetate, and the soluble portion found to consist of PbO and $PbSO_4$ in the molecular proportions of 6 to 1, while the insoluble was PbS in the molecular proportion of 4.

The hard metallic portion was found to contain Pb and S in the proportions of 13 to 6.

These results show that the following is what took place.

(15.) $12PbO + 12PbS = PbSO_4 + Pb_7 + 10PbS + 6PbO + SO_2$

The product was divided into the two portions as follows:—

(16.) $\begin{cases} \text{Slag, } PbSO_4 + 6PbO + 4PbS \\ \text{Metal, } Pb_7 + 6PbS \text{ or } Pb_{13}S_6 \end{cases}$

It will be seen that here again the simple reactions given by the text-books do not represent actually what takes place. I have had opportunities for making examinations of the action of litharge on galena in large quantities over a series of years, and in presence of air, and, without going into all my observations, I may succinctly state the principal stages of the reactions which I have observed.

When a small quantity of PbS is added to a large quantity of melted litharge, the following reaction takes place:—

(17) $24PbO + 2PbS = PbSO_4 + 4Pb + 19PbO +$ (PbS.PbO). The PbS in the right-hand side of the equation is not simply the excess added, because when only 1 molecule of PbS is added to 24PbO, only half the metallic lead is obtained, and still sulphide of lead is found in the residue.

22 grms. PbO + 2 grms. PbS gave 13·6 per cent. of

metallic lead, while the theoretical amount is 14·2. The lead was perfectly pure and soft and contained no sulphide, the whole of the sulphide in the right-hand side of the reaction being dissolved in the oxide, forming (PbS.PbO).

When the products of the reaction ($PbSO_4$ and PbS.PbO) have accumulated to certain extent in the resulting slag, further addition of PbS does not cause any further separation of lead, but simply goes to form a more sulphurous slag. Out of hundreds of experiments, I have built up the following stages to show the direction of the reaction:—

(18.).....$24PbO + 9PbS =$
$3PbSO_4 + 12Pb + 6PbO + 6(PbS\ PbO)$

(19.).....$24PbO + 12PbS =$
$3PbSO_4 + 12Pb + 3PbO + 9(PbS\ PbO)$

(20.).....$24PbO + 24PbS =$
$3PbSO_4 + 12Pb + 9PbS + 12(PbS\ PbO)$

Taking the lead out of the right hand side of the equation, the residue represents different qualities of the grey or black slags of the smelters; but of course the smelters slags contain all the impurities of the ore and the fluxed brick.

On further heating these products they go into further decompositions in two different ways, according to whether they are heated in air or in nitrogen.

If heated in nitrogen, the PbS in solution in PbO slowly acts on the $PbSO_4$ according to the re-action

shaken strong suction is felt by the thumb. This oxidation results in a great increase of the $PbSO_4$ present.

The oxidation may be carried on till a mixture containing only PbO and $PbSO_4$ results. Thus :—

(22.) $5\ PbO + 5\ PbS + O_{17} = 2\ PbSO_4 + 8\ PbO + 3\ SO_2$.

This should give a residue containing 25·4 $PbSO_4$, and the materials should increase by 3·5 per cent.

A mixture in the proportions above indicated, and containing 48·1 per cent. of PbO, and 51·9 per cent. of PbS was melted and allowed to oxidise in a muffle till all the PbS had become oxidised, when the residue was found to contain 26·26 per cent. of $PbSO_4$, and to have gained 3·6 per cent. in weight, thus confirming the above equation.

On melting an intimate mixture of PbS and PbO the reaction assumes this form :—

(23.) $10\ PbS + 20\ PbO = 25\ Pb + 2\ (PbS\ PbO) + PbSO_4 + 7\ SO_2$, yielding 75·54 per cent of lead, instead of 90·63 as required by Percy's equation.

Series of experiments have been made with black slag containing PbO, PbS and $PbSO_4$, which show that when the temperature is high and excess of PbS present, and especially in a reducing atmosphere, the decomposition of the sulphate by the sulphide into PbO, and SO_2 is the principal course of the reaction, and when the temperature is lower and much oxygen present, the direct oxidation of the sulphide takes a major part in the reaction, and as the reaction may be made to assume either of these directions, I need not detail further experiments in this field.

It may be well to note here, however, that, on adding PbS to melted PbO, the first reaction which takes place is most probably :—

(24.) $4PbO + PbS = PbSO_4 + 4Pb$.

But whenever some $PbSO_4$ is formed, the black (PbS PbO) begins to accumulate.

Another reaction requiring attention when examining the course of lead smelting is that of sulphate of lead with carbon, as this reaction is used in hearth smelting of slags.

30 grms. of $PbSO_4$ were fused in a porcelain capsule,

and a piece of charcoal weighing 1.3 grm. plunged into it. Violent effervescence ensued, and the familiar lead fume at once appeared. After the re-action had become very sluggish, the crucible was emptied of the metallic lead, which weighed 8 grms., and it was found that 10 grms. of silicate and 1 grm. of sulphate were left in the crucible—giving a total residue of 19 grms.—while 12·5 grms. were volatilised. The silicate was re-melted, and the carbon pressed into it at a white heat, when it was found that it was scarcely acted upon. It will thus be seen that, during the time required for the reaction (20 minutes), one-third of the sulphate was converted into silicate.

Taking the silicate as metallic lead, the above proportions of fixed and volatile products agree very closely with the equation.

(25.) $24PbSO_4 + 27C = 21Pb + 3PbS_2O_2 + 18SO_2 + 27CO_2$; or,

$24PbSO_4 + 28C = 20Pb + PbS + 3PbS_2O_2 + 17SO_2 + 28CO_2$

as there is generally some sulphur in the lead.

This reaction requires a loss of 12·7 against 12·5 obtained and the lead and its silicate should weigh 18·3 against 19 obtained, but taking off the oxygen retained by the litharge in the silicate the numbers are 18·3 against 18·2 obtained so that the above equation may be taken as correct. It is rather curious that all the sulphide formed in

On heating 6·1 grms. of $PbSO_3$ over the blow-pipe in a hard glass tube it turned black and SO_2 was evolved till the whole was fused, when 1·358 grms. of SO_2 had been given off. This represents 22·2%, while theory requires 22·3 while the sulphate and sulphide were present in the above proportions. On further heating, SO_2 continued to be evolved, but extremely slowly, as during 20 minutes the loss was only increased to 23·7%. Heating was continued for an hour but no trace of lead was formed, the sulphide re-acting on the sulphate to produce litharge as by re-action (21). This is another proof that sulphide and sulphate do not re-act as supposed in the old theory of smelting as they are here produced by a decomposition so that they could re-act in the nascent state, and if such a re-action were possible, it would take place in this case.

When the temperature is raised to white heat, the reaction goes on to its conclusion, and a portion of the sulphide combines with the SO_2 to form the volatile PbS_2O_2, thus :—

(27.)...$25PbSO_3$ yield $15PbSO_4 + 5(PbS\ PbO) + 5SO_2$, while 3 molecules of the PbS go to form the volatile PbS_2O_2 and 3 molecules go to decompose the sulphate, thus :—

(28.)...$5PbS + 6PbSO_4 = 8PbO + 5SO_2 + 3PbS_2O_2$, so that the final reaction may be expressed.

(29.)...$25PbSO_3 = 3\ PbS_2O_2 + 10SO_2 + 13PbO + 9PbSO$.

A very interesting point was noted in connexion with the PbS PbO compound, showing it to be one of the group which contains the volatile PbS_2O_2 (or $PbS\ SO_2$).

I have shown that as soon as the PbS_2O_2 compound passes below a red heat PbS is deposited and SO_2 passes away. That it is pure PbS which is deposited is proved by its cubical crystallisation, and specific gravity, which, when free from oxidation and taken in benzine, is 7·66, while on analysis it gave 13·43 % S, and 86.79 Pb, the theoretical numbers being 13·41 and 86·59.

Now, when PbSPbO is dissolved in PbO and heated to a good red heat, the liquid is seen to be

quite transparent in thin films transmitting red light; but on cooling it becomes black and perfectly opaque, and on dissolving the PbO in acetate of ammonium pure PbS is left. Now this behaviour shows that at a red heat the PbS PbO compound exists, and is a reddish coloured transparent body; but on cooling, the PbS separates out exactly as it does in the SO_2 compound. This also shows that the PbS PbO compound cannot be written Pb_2SO and be considered as a double molecule of PbS (*i.e.*, Pb_2S_2) in which one atom of S is replaced by oxygen, as such a compound would be unlikely to yield PbS on cooling, but would probably be more stable at a lower temperature instead of dissociating. It is curious that these bodies are only stable at a high temperature and split up on cooling, as the contrary is usually the case with gaseous compounds with solid bodies as in the case of Mond's nickel carbonyl.

I have given much attention to this body, and have watched its reactions with galena on the large scale, as my researches have shown that the oxygen compounds of sulphide of lead play an important part in the metallurgy of lead. It was all the more necessary to make a searching study of the matter as Percy, whose commanding position in metallurgy we all respect, has made the following statement in his book: "In metallurgical books which treat of lead smelting in the reverberatory furnace, oxysulphide and

pounds of lead, but that it is due to the power of fractionation of the mixture of lead and its sulphide, or what is in the liquid state subsulphurised lead. We have seen that the re-action (given on page 178), $PbS + PbSO_4 = Pb_2 + 2SO_2$, does not take place, but even were it a possible re-action it *could not* take place in smelting, as the temperature is never high enough to melt either of the constituents of the reaction, and in the solid state they do not react.

When PbS and $PbSO_4$ are very finely pulverised and fused together at the lowest temperature at which they will combine, they react in the following manner :—

$$(30)\ 12PbS + 12PbSO_4 =$$
$$\underbrace{11Pb + 2PbS}_{\text{Hard lead.}} + \underbrace{6PbO + 2PbS + 2PbSO_4}_{\text{Slag.}} + \underbrace{PbS_2O_2 + 16SO_2}_{\text{Fume.}}$$

11·92 Grms. PbS and 15·12 Grms. $PbSO_4$ were carefully melted together and gave :—

		Products. Found.	Products. Theory.		Ultimate Analysis. Found.	Ultimate Analysis. Theory.
Pb	Hard lead.	41·32	42·33	...	38·80	34·98
PbS					6·52	7·35
PbO	Slag.	36·29	37·23	...	18·73	20·56
PbS					6·41	7·35
$PbSO_4$					11·15	9·32
SO_2	Fume.	22·21	20·44	...	18·15	15·78
PbS_2O_2					4·06	4·66
		99·82	100·00		99·82	100·00

V. Lead Smelting.

In applying the knowledge gained in the foregoing pages to the smelting of lead, the behaviour of galena under various conditions has to be kept in view, as the state of the lead ore which is being reduced is an important factor in the result produced.

There are two practical observations which we will find of great use in understanding the re-actions involved in smelting. These are :—

1. The ore is reduced in the rough state—*i.e.*, in small pieces from the size of a split pea to that of a marble, but

the larger pieces decrepitate on heating, so that the sizes may be stated as from a split pea to coarse sand, but the smelters find the coarsest ore easiest to work, and to give the best results. Now, when the fineness is extreme, as in "slimes," the trouble of smelting is enormously increased, and the results obtained are the worst, quite irrespective of the increased impurity in the finer ore.

2. Sulphate of lead is the smelter's worst enemy.

I shall endeavour to show that these two propositions amount to one and the same thing.

When a small piece of galena is heated before a blowpipe flame, a little fume is given off, showing that the sulphur is being burned out, while the surface of the piece will become coated with a liquid like lead, but no sulphate is formed, except a small trace of fume, which condenses on surrounding surfaces and stains them white, giving an erroneous impression that the solid PbS is being oxidised to $PbSO_4$.

On continuing the heating the liquid portion will increase, and this will go on, the solid beneath growing less till all is liquefied.

This liquefaction is not due to rise of temperature, but to the burning out of the sulphur, yielding subsulphurised lead of a lower melting point. When the film of lead is formed on the surface, it dissolves the underlying sulphide, and brings it to the surface

SO_2 and lead, and the products would be entirely lead, sulphur dioxide, and fume.

So much for galena in pieces. Now let us see what goes on when the galena is in very fine powder.

If very quickly heated the liquid portions of the larger particles coalesce and act very much as above, so we need not consider this case. If slowly heated, very little SO_2 is evolved, but oxygen is absorbed, and at the end of an hour the colour is seen to be growing lighter, and after two hours the whole may be converted into sulphate. It is impossible to get the theoretical yield, as some fume is formed in spite of precautions, but by careful roasting I have obtained 115 per cent. of a white sulphate of lead out of a possible 126 per cent., and by taking longer time and a lower temperature higher results might no doubt be obtained.

Now this reaction is the second cause of the formation of slag as the sulphate of lead produced necessitates the use of a higher temperature in "melting down" the charge in the furnace, and thus more oxidation takes place. Besides, the $PbSO_4$ is partly decomposed by the PbS into litharge, and so the galena instead of being converted into lead is used up in two deleterious reactions, first reducing $PbSO_4$ to PbO, and second forming PbS.PbO with the PbO produced. I have proved in several different ways that the reaction, $PbSO_4 + PbS = 2Pb + 2SO_2$ (which is always put forward as the total explanation of lead smelting) has no existence, and does not take place in any circumstances which I have been able to bring about.

But the most complete contradiction to this reaction is given by the lead smelters themselves. The fume which they condense in their long flues is composed principally of $PbSO_4$, so that if the above were true, the best thing a lead smelter could do would be to mix this fume with galena in the required proportions, when, on heating, lead would result. But lead smelters know better than that. Were anyone to propose to put lead fume amongst good ore he would be considered to be mad, or trying a practical joke, as no greater calamity could happen to a smelter. The charge would simply be ruined.

The smelter treats his fume on the slag hearth, mixing it with fuel, and the reaction is that of $PbSO_4$ with C, shown in equation (24.)

With these explanations we will now follow a smelting operation. I have carefully watched the whole operation of smelting many times, and although different workmen use slightly different methods of treatment, I have noted that the main stages are always the same, and these are :—

1. A roasting for two to four hours at a barely visible red heat, during which neither litharge nor sulphate of lead are produced, but sulphur is burnt out, and carries with it some PbS as PbS_2O_2. During this stage no fusion or even pastiness is allowed to take place, and at the end the temperature is generally allowed to decline.

2. The temperature is sharply raised, and lead trickles out as mentioned above, and the slag rapidly forms and runs down on the top of the lead.

The remaining operations are directed towards removing the last traces of sulphide of lead from the slag, but practically no more lead is obtained. The reason for this removal of the sulphide is, that if any is left in, the subsequent treatment of the slag in the blast furnace or slag hearth is rendered difficult, and much fume is produced, whereas a well-oxidised slag is easily treated in the slag hearth.

In order that it may not be supposed that I have ob-

and the whole pushed towards the bridge and back of the furnace. [This has taken about four hours, and there is no separation of lead yet.]

(Stage IV.) The temperature is quickly raised, and lead flows freely, and the whole is melted down into the leadwell.

(Stage V.) Slaked lime [others use lime and charcoal] is thrown in to thicken the slag, which is pushed off the lead, and set up round the sides and left to cool, and again remelted. Sometimes lime is again added and the last treatment repeated. Now all through his work Percy assumes that the roasting forms sulphate of lead, which reacts with the sulphide to form sulphur dioxide and metallic lead, but I have shown that neither by adding sulphide to molten sulphate (or *vice versâ*), nor by fusing any mixture of the two, does any such reaction take place. But there are several other disproofs of any such reaction. In the first place, if it were true, smelters would mix the sulphate fume with ore to aid in the smelting, but any such addition would be fatal to the making of lead. Another disproof lies in the fact that sulphate and sulphide do not react till the fusing point of one is reached, and as that temperature is nearly a white heat, any such temperature in a lead furnace would be entirely out of the question, as it would flux all the brick. Lastly, as the preliminary roasting has reduced the sulphur to 3 to 5 per cent., there is plenty of free lead, so that instead of the $PbSO_4$ and PbS reacting, the $PbSO_4$ would react with the metallic lead, yielding litharge and PbS and diminishing the yield of lead.

I will now examine the process in the light of my researches. It must be remembered that as galena of all degrees of fineness is present, we can only follow the main course of the re-action, as the fine dust of sulphide pursues a different course from the coarse ore, but I assume we are dealing with a coarse ore and that the main re-action is the only one.

In (Stage I.) of Percy's description, the action is entirely one of burning out the sulphur till a sub-sulphurised lead containing from 3 to 5% of sulphur is obtained (or a mixture of 4Pb with one of PbS). In (Stage II.) the material is

placed in a good position for melting down. In (Stage III.) the sub-sulphide is cooled to cause its separation into metallic lead and sulphide. In (Stage IV.) the material is heated to allow the metallic lead to trickle out, and at the same time oxygen is rapidly absorbed, and the compound PbS.PbO formed which flows down as slag. Of course some sulphate is formed at the same time, and the PbS.PbO goes on eagerly absorbing oxygen and forming sulphate. The slag still contains a good deal of galena in fine particles, and requires to be again fractionated, but that could not be done if it were allowed to lie fused on the surface of the lead, nor could the lead itself be purified from the sulphide which it still contains except by being exposed to the air. Another reason for the speedy removal of the slag from contact with the metallic lead is that it has a deleterious effect on the lead if left in contact with it.

When the slag still contains excess of sulphide, that sulphide is gradually transferred to the lead, making it hard; and when on the other hand it is low in sulphide, then the sulphate present is attacked at the expense of the lead by the re-action $PbSO_4 + 4Pb = PbS + 4PbO$, so that it is imperative to at once separate the slag from the metal, which is done in Stage V.

But it is not ready to be treated in the blast furnace or slag hearth, as it contains much sulphide, so the only thing to be done is to "dry" it up by admixture of some neutral

evolution of sulphur dioxide. This clearly proves that the main source of the metallic lead is simply fractionation of the partly desulphurised sulphide, as illustrated on page 173, and not a double decomposition.

Of course the experiment on page 173 did not include oxidation, but was executed in small steps for the special purpose of illustrating the gradual fractionation of a low sulphide into lead and its mono-sulphide.

Percy mentions at some periods of the smelting a sound is heard—"sizzling," I think he calls it—showing an evolution of gas. I have often heard this, and have imitated it in large flat basins in a muffle furnace, and I have often found it to be connected with the action of sulphide of lead on the sulphate of lead dissolved in litharge resulting in the formation of more litharge as shown in equation (21).

It is also caused by the decomposition of the PbS.PbO compound when suddenly subjected to heat, as SO_2 is evolved and a sub-sulphurised lead produced thus:—

(31.) $2\ (PbS.PbO) = Pb_4\ S + SO_2$.

A practical proof that the reactions of the oxidised lead compounds upon the PbS are not the cause of the production of the metal, lies in the fact that iron rabbles are used constantly stirring up the materials, and iron is attacked by PbS at as low a temperature as that at which it reacts with the sulphate of lead, so that, if the temperature were raised to bring about a reaction of the PbS with the $PbSO_4$, the iron rabbles would be quickly destroyed. I have shown that when the temperature is raised the SO_2 evolved begins to carry off much PbS by forming the volatile compound PbS_2O_2; so the great aim of the smelter is to obtain a burning out of the sulphur at as low a temperature as possible, and it is wonderful how quickly the sulphur may be burnt out at a heat below visible red; in fact, sulphur seems to be much more loosely combined with lead at low than at high temperatures, as is shown by two experiments. I have often noticed that when pure sulphide of lead, such as that obtained by sublimation, is quickly heated in a glass tube, in an atmosphere of nitrogen, a minute quantity of sulphur is evolved and condenses in the tube. Then, we

have seen, on page 183, that, at a temperature of 350° C., liquid SO_3 acts upon PbS, forming PbO and setting free sulphur, whereas, at a white heat, when its oxidising power might be expected to be greatly increased, it does not oxidise the PbS at all, but forms a compound from which pure PbS is deposited at a lower temperature.

The last point to be noticed is, that at the moment of heating the roasted ore to smelt down the lead, there is a a great absorption of oxygen to form the compound PbS.PbO, and that the formation of this compound—the basis of black slag—causes the rapid expulsion of a nearly pure lead. I have often repeated that experiment in a model furnace, but at the moment of melting down I have replaced the air by nitrogen, when the separation of lead was not at all so sharp, and it required a second cooling and remelting to get anything like the same result.

To sum up, then, the method of smelting lead in the reverberatory furnace is not (as hitherto explained) a result of the equation $PbS + PbSO_4 = 2Pb + 2SO_2$, which I have shown to have no existence, but it is simply a burning out of the sulphur by air, producing a sub-sulphurised lead which, on being allowed to solidify, separates into Pb and PbS, and on re-melting this, the lead melts out first, leaving the more infusible PbS, while oxygen is absorbed and PbS.PbO formed, which, along with all the impurities in the ore, silica dissolved from the brick

heating, and cooling, &c., with the result that the same ore may yield from 60 to 67 per cent. of lead, according to manipulation; but, of course, the slag of the smelter, who has made the smaller quantity of lead, will yield a proportionally greater amount of lead on reduction on the slag hearth besides being more easily reduced.

We will now consider the slag. Percy was much puzzled to find any reason for adding lime to the slag when "setting it up" from the lead in the lead well, as the lime does not appear to react with the slag.

It is said that in Spain they use a mixture of lime and coke brise, and the coke doubtless reduces a little of the PbO to metal; but lime is always used.

The reason for adding the lime is two-fold. It acts in the first place as a stiffener or "drier," which enables the workmen to "set up" the slag and separate it from the metal. But its real use is in the subsequent smelting of the slag. Percy has shown on page 235 that the slag gradually accumulates combined Silica, and that this may rise to 12 per cent. of the slag. It sometimes rises much higher with "slimes," or ores containing much Silica, or if the workman allows his slag to lie too long liquid before "setting it up," as it then attacks the furnace bottom. Now silicate of lead, as shown in experiment (25), is not reducible to any extent by carbon, but on adding lime, the stronger base lime takes the silica and throws out the lead oxide, which is at once reduced by the carbon of the slag hearth. I need not treat of the reactions of the slag hearth at any length, as they are very simple. The slag consists of oxide, sulphate and silicate of lead, as the last two hours' work in the reverberatory furnace is practically a process to get rid of the last traces of sulphide to render the slag fit for the slag hearth. The oxide is reduced at once, the silicate split up by the lime, and the sulphate is reduced as shown at equation (25), generally according to the second of these equations, as the lead obtained is a little "hard" from the presence of some sulphur.

Lead fume from the flues, consisting mainly of sulphate of lead, is smelted in the same way with carbon in a hearth

or cupola; but being a very fine powder, not only is fume produced by the PbS_2O_2 reaction, but a quantity is mechanically carried away by the blast. I have described these reactions as succintly as possible, and the whole operation in the reverberatory furnace may be summed up in the reaction.

(32.) $24PbS + 44\ O = 17Pb + 2\ (PbS.PbO) + PbSO_4 + 2PbS_2O_2 + 17SO_2$ which would represent the best results obtained—viz., 66 per cent. of lead, 12 per cent. slag, and 6 per cent. fume, but the slag often amounts to 14 or 16 per cent., and the fume to as high as 12 per cent., according to the nature of the ore. But it must not be thought that there is a complete cut and dry chemical reaction like that represented at (30), because, according to the fineness of the ore, the air may be desulphurising it or converting it into sulphate, but only the very finest particles go directly to form sulphate, and with clean coarse ore the above reaction practically represents what takes place.

The new compound PbS_2O_2 is very easily oxidised (if mixed with excess of air or oxygen) to sulphate of lead and sulphur dioxide, and as the oxidation when conducted by air takes place in a large volume of a neutral gas—nitrogen—the particles of $PbSO_4$ are almost molecularly fine if cooled before they have time to coalesce, hence the great difficulty of condensing lead fume. It will be seen

furnace, I have found it impossible to materially increase the yield of lead by reverberatory treatment. The formation of the two oxysulphides which constitute fume and slag are inevitable with such a process, but these researches have shown that there are other means of treatment, rendered possible of execution by the invention of the basic Bessemer process for iron, by which the oxysulphide slag may be entirely avoided, while the other volatile oxysulphide may be made a useful step in the process instead of being considered a troublesome accident.

6. Proposed New Metallurgy of Lead.

The new metallurgical treatment of galena may be roughly stated as follows:—

The galena is placed in a basic lined converter in the molten state and air blown on its surface as well as a little forced through it to obtain as much volatilisation as possible. One half of the lead is volatilised and converted by excess of air into sulphate of lead. Should the lead be sufficiently rich in silver to render it advisable to obtain a concentrated silver lead the greater part of the sulphate is returned to the metallic lead in the converter and air forced through the lead to maintain a high temperature, and induce the reaction by which litharge is produced, as on page 187. By using rather less than the quantity of sulphate required, and forcing excess of air, the sulphide which forms as shown in the secondary reaction on page 187, is entirely oxidised, and a good litharge obtained. The small quantity of lead in excess of the reaction will now be found to contain all the silver. Should the lead be poor in silver it may simply be run off and a new charge prepared.

Should it be required to convert all the galena into the metal, the sulphate may be added to a fresh charge of galena when the reaction (I.) takes place. The reaction there given represents the first stages of such a decomposition, and the volatilization is extreme; but the metallic lead produced dissolves in the sulphide and lowers the melting point, and if the temperature be allowed to fall,

the reaction approaches more nearly a burning out of the sulphur by the oxygen of the sulphate; so by this process the final result of the action of an insufficient quantity of sulphate is to produce a low sulphide, which may be then finished off by air, and so metallic lead produced.

This process is capable of many modifications, but by its means the galena may be turned into the metal, or its oxide or sulphate in any proportions, all that is required being a basic lined converter and a series of wet condensers in which to collect the various sublimates, whether of white sulphate or of black sulphide.

The sulphur dioxide is obtained in a concentrated state and quite free from any association of lead, so that it can be utilised at the same time in other manufactures. As these reactions are capable of being quickly carried out, and in large quantities at a time, they form the basis of a new metallurgy of lead, which will hold the same relation to the old method as Bessemer's process does to the old hand-puddling of iron.

It may be wondered how the old text-book reactions came to be accepted, although they are quite erroneous.

Most of the experimenters wrought with coke furnaces and fireclay crucibles, and they found, as Percy did, that sulphide of lead passed freely through the pores of the crucible, so that a considerable loss had to be expected. Plumbago crucibles at once reduce litharge to metallic lead. Now in conducting an experiment, it seems to have been the habit to place the acting substances in a crucible and heat it in a coke furnace till the reaction was complete. The litharge was largely absorbed by the crucible, and sulphide of lead passed through the pores, while all sulphate was decomposed and converted into silicate. Hence the only substance left unchanged in the crucible was metallic lead, and it will be seen, by comparing my formulæ with those of the text-books, that the latter err in stating that the reaction yields metallic lead, when it really yields a mixture of this with litharge or sulphate. As the experimenter was prepared for a large loss, no notice was taken of the

working in thin salamander or Berlin porcelain vessels, I have been enabled to heat the materials quickly to the required temperature and watch the re-action, and instantly raise or lower the temperature. Again the studies have sometimes been made at temperatures which are incompatible with lead smelting. For instance, PbO is the only substance except lead which is liquid at the temperature of the smelting furnace; on adding PbS to it the substance PbS.PbO is formed, but on mixing them in the proportion $PbS + 2PbO = 3Pb + SO_2$ and placing them in a Hessian crucible and heating for an hour in a coke furnace, there is found to be only metallic lead remaining. But no such reaction takes place in the reverberatory furnace, as the temperature is not sufficiently high. But as Percy says, only rough approximations were expected,* as the existence of the volatile PbS_2O_2 was unsuspected.

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—is Mr. Hannay satisfied that nascent sulphate of lead is not formed in the treatment of galena, which would re-act on the lead sulphide, and produce metallic lead which would be gradually absorbed by the still undecomposed sulphide? Probably this will account for the fact that on raising the temperature of the charge in an ordinary smelting operation, metallic lead flows without any further copious evolution of SO_2. Now, it is almost impossible to reproduce the effect of any of these nascent reactions in crucibles by adding mixtures, but you can get at it in an indirect way as nearly as possible; and what makes me think there is something in my remark is this: You take sulphate of lead and carbon in such proportions that the resulting mixture at a low temperature will contain sulphide of lead and sulphate of lead in exact molecular proportions, and favourable to bring about a reduction of each other, producing lead and SO_2. Now you elevate that temperature, and you get metallic lead. In the case I have just quoted there are the exact proportions; on the other hand, if you take three equivalents of your sulphate of lead and two of carbon, you produce a mixture at a low temperature which contains twice the requisite quantity of sulphate to bring about complete reduction of the mixture into lead and SO_2. And what do you get? Oxide of lead and SO_2. In the reaction represented by the first proposition, we have been able to

conditions have been maintained that exist in the lead smelting furnace. I think Mr. Hannay will admit that adding molten sulphate to molten sulphide in no sense represents the action that takes place in the lead furnace. Then following Mr. Hannay's blow-pipe experiment, which is most instructive: is it not possible that sulphate of lead may be formed in a nascent condition, and at its birth do its work? If you do not find sulphate of lead that is no proof to me that sulphate of lead had not been formed. Then, referring to the re-action that Mr. Hannay notices under his blow-pipe, I wonder if he took a bit of the galena and thoroughly examined it under a microscope, and did he not notice lead right through the whole mass? I think he would have done so. But although he makes a strong point of the fact that no sulphate of lead was detected after his blow-pipe operation, I am of opinion that it was still formed, but instantly decomposed by the sulphide of lead. Mr. Hollway suggested the reduction of sulphide of lead when he was working his copper experiments by blowing air through molten sulphides, but I do not think he ever attempted to touch this sulphate question at all.

Mr. Vautin then put the following reactions upon the black-board, illustrating the remarks he had made as to the importance of the molecular proportion of mixtures for bringing about the perfect decomposition of sulphide by the sulphate:—

$$2PbSO_4 + 2C + PbSO_4 + PS + 2CO_2$$

Mr. J. H. Collins: Mr. Hannay has told us—and we knew it before—how difficult it is to condense lead fume. Now, in this new metallurgy of lead, where the fume is made in as large a quantity as possible, and the operation repeated over and over again, would not the loss, instead of being 7 per cent. or 9 per cent., be something fabulous? However valuable the condensed portion of the fume might prove to be in the market, would not the absolute loss be so great that the process must be a bad one? It may be that one of the things which Mr. Hannay could not find room to put in his paper is a description of some more perfect con-

denser, but so far as I know anything of the condensation of lead fume, I should think the new metallurgy would lead to an enormous loss. There is only one other remark I would make, and that is to call attention to the passage in which Mr. Hannay speaks of the effects of the rule of thumb method of smelting, and which ought, I think, to make us feel very humble. Not only does he seek to show us that Dr. Percy and all his researches were wrong, but he tells us that now he has found out the truth he is not able to show where the process handed down from father to son can be improved. In face of that, I think we ought to feel rather modest in our criticisms of rule of thumb men.

Mr. Thomas Rickard said he did not know, how far the chemists who were present would be reconciled to this wholesale demolition of almost everything that Dr. Percy had taught in the reactions of lead metallurgy. The paper began by supposing that one had a product which was first made molten and then poured into the converter; now sulphide of lead being a most refractory substance, could not be got into a molten state unless it was calcined. With the calcining and the fusion as a preparation the new process did not work in the direction of simplification. With regard to the slags, they were relatively not very important, for, in the case of rich galenas carrying, say, from 70 to 80 per cent. of lead, the re-

his paper, says that Dr. Percy speaks of "supposed sub-sulphides of lead." It is perfectly true that he has headed a paragraph in that way, but the manner in which Mr. Hannay puts it is apt to be misleading, inasmuch as it would give one the impression that Dr. Percy believed in sub-sulphides of lead—at any rate, to some extent; whereas one has only got to read Dr. Percy to see that he believed nothing of the kind, and that he was at some pains to prove the non-existence of sub-sulphides of lead. On page 39 of Dr. Percy's well-known work on "Lead," one will find it very clearly shown that sub-sulphide of lead does not exist, and for my own part I have never supposed that it did. Then, as regards the specific gravity of lead sulphide and the oxidation of galena, on page 47 of Dr. Percy's book, it is stated that Rodwell, who was a very careful experimenter, had found that sulphide of lead oxidised very freely, and that point is gone into. Now, Mr. Hannay tells us that he cannot take the specific gravity of sulphide of lead owing to oxidation; but it is perfectly clear that water *per se* would not oxidise sulphide of lead, so that if you remove the oxygen from the water by boiling and then take the specific gravity it is very difficult to see how the oxidation of the sulphide of lead could possibly take place. It cannot be pretended for a moment that water at that temperature would oxidise the sulphide of lead. Unquestionably if the air had access to the lead it would oxidise, as shown by Rodwell and others. Then, as regards the furnace reactions, Mr. Hannay states that "Percy says that the chemistry of lead smelting is unexplained," but a line or two further on he says, "For instance, $PbS + PbSO_4 = Pb_2 + 2SO_2$ is given as the whole *rationale* of lead smelting." These two statements hardly seem to coincide. If this is the whole *rationale* of lead smelting, then how is it that lead smelting is unexplained? I think I shall re-echo the feeling of all old School of Mines men when I say that I have the greatest possible respect for Dr. Percy and his work. Those who had a knowledge of him admitted him to be, as far as care and accuracy are concerned, the greatest experi-

menter we have ever had. He spent endless time, and spared no expense or trouble in getting at the absolute truth, and he never attached his name to anything as a fact which he had not proved down to the ground, but always gave it with all the necessary reservation. He always gave the data upon which he founded his conclusions, and described exactly how the experiments were performed, so that I should be very loth, indeed, to reject any of Dr. Percy's statements without the fullest and clearest proof. There is no question that the reactions of lead smelting are extremely complicated. They vary with temperature, and with slight variations in composition, and the addition of any substance whatsoever, even in small quantity, will alter altogether the course of the reactions. Mr. Hannay himself has admitted and shown, in fact, that lead smelting is extremely complicated. He has shown us that at different temperatures the reactions alter; but I do not think he has taken sufficiently into account that this fact may go even further than he himself has carried it. As regards certain experiments he made with liquid sulphurous acid, it does not appear to me that the question of vapour tension and dissociation have been sufficiently taken into account. I think that if they are taken into account we shall see that the explanation is perfectly simple, and in some cases somewhat different to that given in Mr. Hannay's paper; but the whole paper is so extremely com-

at the date of the publication of Dr. Percy's book. He himself refers to it at page 50 and page 37. It does not appear from the statement in Dr. Percy's book that he or others at that time appreciated the fact, if it be the case, that the sulphide of lead forms a definite compound with water vapour, carbonic acid, and other gases, and is thus volatilised as a compound, but that sulphide of lead was volatilised when brought into contact with water vapour was known. The fact appears from Mr. Hannay's experiments that on lowering the temperature this body breaks up. He further says that the compound of sulphide and oxide of lead also breaks up on lowering the temperature. It suggested itself to me whilst listening to him whether this volatile compound of a gas and a solid—the result of the action of carbonic acid on sulphide of lead — was not the case of the solution of a solid in a gas. It does not appear an impossible case, although it is not one with which we are familiar. Mr. Hannay himself has told us freely that the work he has done has not advanced the practical side of the question one iota. Whatever may be the case with regard to the reactions, the practice remains the same, except in so far as he proposes what he calls a new metallurgy of lead. Now, the reactions which he proposes are quite possible doubtless on a certain scale, but one is brought face to face with the question of condensation. The question of the condensation of lead fume has always been looked upon as one of the most difficult problems in metallurgy. It is one which gives immense trouble at present, and I think I am quite safe in saying that it has not been satisfactorily solved on a practical scale. I would therefore ask Mr. Hannay to tell us how he proposes to condense on such an enormous scale the fumes which he proposes intentionally to produce, and which, when unintentionally produced in the present method of smelting, gives such an enormous amount of trouble. As regards the furnace in which these operations are to be carried on — the Bessemer — he tells us that it is to be lined with basic material, but I should

be very much surprised, indeed, if he could find for his purpose any basic material which would not be greatly corroded by the oxide of lead. Oxide of lead acts vigorously on most oxides, in fact, I might say on all to a certain extent, and it should be recollected that one has not got sulphide of lead pure and simple, but the ordinary sulphide of lead of the smelter, which contains slag and other things which would materially assist in the corrosion of the lining. As to rule of thumb, it is simply the outcome of experience, and is really the nature of science; it is the outcome of the careful observation of competent men, and not of "Tom, Dick, and Harry." It is the sum of the observations of many men who, although they may be in humble positions, have good brains and accurate powers of observation. The Bessemer process itself, which is supposed to be the father of this process, was no rule of thumb process; neither was the Siemens process, and these are the two greatest processes at the present day used in the manufacture of steel, which is the greatest metallurgical industry we have. Again, in the treatment of silver, we have the Claudet process, which was invented by the father of a member of our Council. That is a most scientific and extremely valuable process, and no one without considerable scientific acquirements could have possibly invented it. The steam engine of the present day is the outcome of science; it never was a rule of

experiment showing the production of lead by the interposition of carbon does not bear on the theory of lead smelting, as carbon is not present during smelting, and its presence would be deleterious. Besides this, carbon acts in a curious way, as is instanced by heating $PbCl_2$ with carbon in presence of some air when metallic lead results, probably from the formation of oxychloride and its reduction by carbon, $COCl_2$ being given off.

The litharge present in smelting arises from the oxidation of the lead set free, and this formation of litharge is the beginning of the formation of slag. In reply to Mr. Collins as to the condensation of fume, a condenser was invented some years since which is absolutely perfect, and condenses 99.99 per cent. of lead fume in a space of 20 cubic feet. In treating 10 tons per day only 24 lbs. is lost by passing the condenser. In reply to Mr. Rickard, reference was made to the work of Dr. Rosing, who has shown that by passing air through molten lead it can be purified from antimony, arsenic, &c., and very hard lead rendered beautifully soft : in fact, analyses show 99·99 per cent. of lead, and only 2 ozs. per ton of impurities after this treatment. By the new treatment proposed the work is reduced to a point of complete simplicity, there being no roasting or preliminary treatment, and the fusion of the galena is carried out by the heat of its own oxidation, so that all that is required is galena and air, and instead of treating one ton at a time and taking eight hours for treatment, ten tons are treated at a time and the operation is complete in under one hour. In the production of the pure volatile compound PbS_2O_2, there is a very minute volatilisation of silver less than occurs in smelting. Of course, in the presence of arsenic and antimony gold is carried over, but the cases of galena carrying much gold are rare. Then what we call rule of thumb is simply the result of clever men using their brains by observing how the best result can be arrived at, and they are as much inventors as trained chemists, the main difference being that while the clever workman may give you a good process, the chemist tries to throw light on the *rationale* of that process, and to

clear the ground all round it, so that the knowledge so gained will serve to point the way to improvements in the future. In short, while the present position has been gained by rule of thumb, the future progress is moulded by the work of the scientific investigator. But many of our greatest industries are entirely due to the purely scientific researches of the trained chemist. In reply to Professor Huntington, I purposely quoted Percy's heading "Supposed sub-sulphides of lead" in order to show that he (Percy) did not believe in their existence. Although Rodwell pointed out the oxidation of galena in powder when wet, he did not give the true specific gravity which was my object. Percy simply mentions one formula $PbS + PbSO_4 = 2Pb + 2SO_2$ as the only explanation of smelting, and then throughout his whole book he shows that the whole matter is little understood, and he calls for further investigation. Although the paper shows that the furnace re-actions of lead are extremely complex, these re-actions do not take place in a lead furnace, and the action of smelting is really in the main very simple, and consists in burning out the sulphur and then fractionating the mixture of lead and its sulphide into its two terms. Of course sulphate of lead is smelted, but generally by the ore hearth or cupola furnace with carbon. The compound PbS_2O_2 may be a solution of a solid in a gas, but as its composition seems to be independent of temperature it looks more like a regular

there was twice as much sulphate as was necessary lead was not formed.

The PRESIDENT: If we have learned anything to-night, it is that the complexity of lead smelting still continues.

SIXTH ORDINARY MEETING.

The Sixth Ordinary Meeting of the Third Session was held on Wednesday evening, 16th May 1894, in the Lecture Theatre of the Geological Museum, Jermyn Street, S.W., at 8 o'clock.

Professor A. K. Huntington (President) occupied the chair.

The minutes of the last meeting having been read by the Secretary and duly confirmed, the discussion on Mr. Garland's paper was then concluded and replied to.

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NICKEL MINING IN NEW CALEDONIA.*

DISCUSSION (*Concluded*).

Mr. R. H. Williams contributed the following remarks, which were read by Mr. J. H. Collins:—

The subject is full of interest to me, having been the largest producer of nickel ore in Europe, and associated and interested in the trade for some eighteen or twenty years. Between 1852 and 1860 the price of refined nickel varied from 10s. to 7s. per lb. The nickel ore I raised and sold from the S. Austell Consols Mines realised £80 to £84 per ton. This same ore now would only realise £15 or £20 a ton. The mines producing this ore in Cornwall previous to the discovery made in New Caledonia were the Pengelly Mine, the Fowey Consols Mines, and the S. Austell Consols Mines. In Scotland the only mine I know of was one in the domain of the Duke of Argyle—the ore, magnetic pyrites, producing some 3% of nickel. It is a remarkable fact that so early as 1790 D'Entrecasteaux visited New Caledonia, and in his description says: "The inhabitants of New Caledonia are not acquainted with the use of the bow but are armed with darts, slings, and clubs, which they make with a great deal of care. Besides fish, which is their principal food, they eat a kind of large spider. The land being in general barren, to appease hunger the men eat a sort of greenish soft steatite." I am of opinion this greenish soft steatite was the silicate of nickel seen in stripes down the sides of the mountains. I have now by me specimens of green indurated clay, rich in nickel, and it is most remarkable it should have been eaten by the natives. At the same time it shows clearly that silicate of nickel can have no injurious effects on the human system. The scientific members of the French Commune that were sent out to New Caledonia as convicts soon made themselves acquainted with the contents of this greenish soft steatite, and the result has been to, I think, close all the other nickel mines in Europe.

* A paper read on February 21st 1894. See page 121.

Mr. S. Herbert Cox then read Mr. Garland's reply, which was as follows :—

I regret very much that a long illness has prevented my replying to the discussion on my paper, on "Nickel Mining in New Caledonia," at an earlier meeting, and that even now I have been obliged to narrow down my remarks to a degree that will, I fear, cause disappointment to many of the members who took part in the discussion. I trust, however, that the circumstances of the case will ensure the indulgence of all, and will be regarded as some excuse for the incompleteness of my reply. Mr. Cox asks whether any deep sinkings have ever been made to determine if the deposits of nickel ore which have died out and passed into silicate of magnesia occur again at a greater depth. No such case came under my notice, and, I believe, no such desirable trial has ever been made.

No copper occurs in the nickeliferous serpentine district; but this metal occurs and has been worked at Balade in the extreme north of the island, and is there met with, I am informed, in the schist formation. Quite a variety of copper ores have been found there, but I believe the silicate does not occur.

I have been twitted, in a friendly way, by one or two speakers for my seeming inconsistency in objecting to the two names Garnierite and Noumeaite being applied to what I regard as the same mineral, and then, having the temerity

It must be remembered that the relative specific gravities of the ore and its gangue (serpentine) does not admit of water concentration, and that the only concentration possible is that described in the paper—viz.;—dry dressing, cobbing, screening, and hand-picking. Undoubtedly, if the cobbed and hand-selected portions only were utilised as a marketable product, the percentage of the ore would be something like doubled; but the crude smalls which form the bulk of the ore and which does not admit of concentration at all, are mixed with the richer portions, and thus the percentage of the metal is reduced to the average which, I am informed, is about 7 or 8 per cent.

As to the wire rope tramways: for mountainous districts, I do not think there is any means of transport which is at once so economical, so effective, and so universally applicable. Mr. Rickard has given us a lengthy and useful speech, to reply fully to which would inflict another paper upon this meeting which I shall not be so indiscreet as to attempt to do. I do not find in my paper the statement which Mr. Rickard attributes to me, that the only public company in the island is La Société le Nickel; my statement was that this company, though not the only nickel mining company in the island, is by far the most important. He further states that all the ore is not in the hands of the Nickel Company. My information, obtained on the spot, is that the Nickel Company are the purchasers and the shippers of all the nickel ore raised. I understand, however, that the managers of some of the mines are not satisfied with the prices they obtain from that company, and that they talk of shipping their ores direct to Europe. Mr. Rickard states, on the authority of friends of his, that the mines are capable of producing not 60,000 but three times 60,000 tons per annum, and that they have produced as much as 100,000 tons. I make no statement in my paper as to what they are capable of producing. No doubt they could raise much greater quantities if they were to work more mines and employ a greater number of men, but what they were actually raising at the time of my visit was, roughly

speaking, 60,000 metric tons. In 1891 the total quantity produced was 61,426 metric tons, and for the half-year ending 30th June 1892, the total raisings were 34,557 metric tons, or at the rate of 69,114 tons for the year. These figures may, I think, be taken to be quite reliable, as I obtained them from the highest authority—the Mining Department at Noumea. I was, moreover, given to understand by various mining men that those figures had never before been reached in any one year. My information, as will appear from my paper, extends only to the autumn of 1892 when I left the island; perhaps Mr. Rickard's information is of more recent date.

Mr. Rickard is quite right in assuming that I have no desire to do injustice to the natives. My remarks —which were, I believe, well-grounded—applied, however, to the natives of New Caledonia, while Mr. Rickard refers to those of the Loyalty Islands, who are, I understand, good workmen. It is edifying to note the off-hand way in which my friend disposes of the question as to how the nickel deposits occur almost invariably in elevated positions; but when he tells us it does not require much ability to account for it, and proceeds to inform us that the ore has not got up there, but that it is simply a question of denudation, one feels that the explanation does not quite cover the ground—it does not, for instance, throw any light, as Mr. Geo. Collins points

are met with in such a number of places over the immense serpentine areas, there is no probability of their being exhausted for a very long period.

Some promising seams of coal have been discovered, notably at Dumbea, where they have been exposed by day levels, but no attempt to work coal on a commercial scale had been made in the island up to the time of my visit. I think we must all appreciate the useful information Mr. Rickard has contributed to the metallurgy of nickel.

Mr. Fowler asks if I know of any analagous deposits to those of New Caledonia. I understand that similar ores are found in two or three places in the United States, as in North Carolina and in Oregon, and that they occur in connexion with serpentine rock. I have seen a collection of specimens of the ore from Oregon and the difference between this ore and the Garnierite of New Caledonia is scarcely distinguishable. As far as my information goes New Caledonia is the only place which has hitherto yielded the silicate ores of nickel in important quantities. The sulphides and arsenides of nickel are not met with in the serpentines of New Caledonia. The outcrops are generally simply green coatings of low grade nickel ore on the weathered serpentine rock.

As to the origin of the deposits I think they are probably segregation veins, and inasmuch as the enclosing rock is found to contain 0·25 % to 0·75 % of nickel, there can be little doubt, I imagine, that the filling of the veins and joints has been due to lateral secretion, or the leaching out of the metal from the serpentine, and its subsequent deposition in the form in which it is now found. As to the origin of the red earth which Mr. George Collins inquires about :—of the two theories he mentions I should be inclined to adopt that of Levat—that it is an alteration product of the serpentine. The cobaltiferous wad, locally termed "Cobalt terra," is being worked to a very limited extent. It contains, on an average, about 3½ per cent of the oxide.

I do not know if Mr. O'Donoghue intended to convey that it is a common practice to shift the cable tramways from place to place when the vessels come round the coast

to be loaded. No case of this sort came under my observation, or to my knowledge, and seeing that a large proportion of those cables are fixed in almost inaccessible places, I should have imagined it would, as a rule, be impracticable to adopt a plan involving so much labour, loss of time, and expense. The double line aerial tramways referred to in my paper cannot certainly be rightly described as "little simple things which are moved from one place to another."

Mr. Kitto contributes some interesting remarks on the metallurgical side of the subject which my paper does not profess to deal with. It only remains for me to thank the members for the kind way in which they have received my paper.

The CHAIRMAN: I am sure our thanks are due to Mr. Garland for his interesting paper, which is one of considerable value to the Institution—a sort of paper that we want more than any other, because it is the outcome of investigations carried out on the spot, and the discussion has also been of a very interesting character. From the work of Mr. Claudet, I think it is pretty clear that the chocolate ore, or "chocolite," as Mr. Garland describes it, is nothing but a mechanical mixture of oxide of iron and silicate of nickel. The result of treating this material with hydrochloric acid, as performed by Mr. Claudet, shows that, I think, very clearly. There was a specimen shown

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THE SAMPLING OF ORES AND TAILINGS.

By Thomas Clarkson (*Member*).

One of the most frequent operations with which the metallurgist or mining engineer is concerned is the assaying or analysis of raw material and products. Great importance is necessarily attached to this operation, supplying as it does the quantitative basis upon which has to rest all calculations relative to the determination of values, or relative to the question of economical treatment, the amount of loss, the degree of fineness, &c. Assaying is something of an art but is also a good deal of a science, having attained to a very high degree of precision, and necessitating the use of the most delicate and sensitive instruments for the measurement of weights that can be devised or constructed by the refined mechanician. The saying that "A science advances only in proportion to its power of accurate measurement," is now so familiar as to have become almost a platitude. It is not easy to imagine what was the condition of the science of metallurgy, if science it could then be called, when assaying was scarcely known, or but little practised. It is clear that intelligent work would be as unlikely as for a man to safely traverse a dangerous pathway by night without the aid of a lamp. Under such circumstances success could be had only by sheer luck. Yet, even since the art of assaying has supplied "a lamp above ground" to the miner, it has happened, and not very long ago, that a deposit which was worked unsuccessfully for copper, was proved by other workers to be a gold mine. If a proper analysis had been made, such a mistake would not have

been possible. This case is probably not very exceptional as an illustration of the insufficient practice of assaying.

We are now led to ask, what are the reasons for this insufficient practice of assaying? There are probably several reasons for this; one is undoubtedly the lack of appreciation of scientific methods of work, which is doubtless owing to deficient technical training. Perhaps another reason may be that some managers love darkness rather than light, because they fear that the process they are working is not quite so efficient as they would like to believe; and if the search light of the assay were focussed upon all their products, it might reveal an unwelcome condition of things. Perhaps this reason is exceptional. Another reason is, that the preliminary operations to assaying have not yet been brought to such a degree of perfection as in every case to place the assayer's report above doubt and suspicion. For example, it may be stated, in a prospectus, that the assay of a portion of quartz from a certain mine is 1,000 ozs. of gold to the ton, and this may be certified to by one of the distinguished members of this institution, whose name would be of course quite sufficient guarantee for a *bona fide* assay. Yet this report is generally of no practical use as an indication of the value of the property, simply because there is no guarantee that the portion of quartz is an average of what there is in the mine; in other words, the portion is not representative and

selection combined with laborious hand mixing. This is a sufficient explanation of variations and errors, for the human machine is, as every one knows, peculiarly subject to variations, and to the influence of external forces which may be either conscious or otherwise. There is no reason why hand selection should not be abolished for most sampling purposes; in fact, with one or two exceptions, there appears to be every reason why it should be altogether abolished, when the work can be done by machine with uniform accuracy and with less than a tenth of the former expenditure of time and money. Sampling by machine is not a new idea in America, although it is a novel practice so far upon this side of the Atlantic. With that mechanical spirit, which is so characteristic of them, the Americans have designed several machines for sampling, which have been used for some time, and although not free from defects, they have proved a great advance upon hand work as to general convenience, rapidity, and efficiency.

In the machine before you, the defects of former machines have been eliminated. It has been tested by American mining engineers, even by some who are interested in the construction of other samplers, and the unanimous conclusion has been that this machine is a great advance upon anything they had, and that its operation is practically perfect. As to this, the machine will presently speak for itself by a practical demonstration, and in a far more eloquent manner than I could possibly attempt. Some of the members of the Institution are familiar with the machine, but in order that the practical tests may be intelligible to all, it is desirable to explain the general construction and action of the machine, as well as the nature of the proposed trials.

Going to the first elements of machine sampling, suppose that we have a fixed hopper delivering a stream of ore. This stream may be divided by a vertical plate into two parts, but as the contents of the hopper may be richer upon one side than on the other, we cannot be certain that the two parts into which the stream is divided are of the same value and composition. Something more is necessary to

ensure this identity of composition. This something is supplied by giving the hopper a rotary motion, so that however variable may be the value of the ore in different parts of the hopper, each part comes in turn over each side of the dividing plate. If the edge of the plate passes exactly through the centre of the rotating stream of ore, then we may be confident that the two portions of ore are of the same composition, and further, they should be of practically the same weight. Each of them would be a 50 per cent. sample, but this is much too large for ordinary purposes, therefore something else is necessary which will enable a smaller percentage to be taken without unnecessary repetitions. Your attention is now asked to a little elementary plane geometry. If we make a drawing of a section of the rotary stream, it would clearly be a circle, and the edge of the dividing plate would appear as a diameter of the circle. Every circle contains, according to current geometrical notation, 360 degrees, and a diameter divides a circle into two portions each of 180 degrees. Two diameters at right angles divide up the circle into 4 segments, each of 90°, and similarly, 5 diameters equally spaced split up the circle into 10 segments, each of 36°. Now consider what would happen if a rotary stream of ore were divided by a number of plates arranged radially from the centre of the stream, so as to make angles of 36° with one another, it is clear that the

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this, too, without increasing its angle. The obvious solution is to make the material spread out to a large diameter, in other words, instead of a solid stream to use a hollow one, which can be expanded to operate upon any required width of segment. This spreading out is easily effected by placing a cone, so that its point splits centrally the solid rotary stream as it is delivered by the hopper. We have now collected from first principles all the elements of a perfect sampling machine, and all that is necessary is to design an apparatus which will embody all these elements. Such a machine is the one before you. It is provided with a large storage hopper above the rotary hopper, and also with an arrangement by which the cutting segments can be rapidly changed from 36° to 18° or *vice versâ*, so as to take either 10 per cent. or 5 per cent. samples. Care has been taken to prevent the retention of any material in the machine, by giving all the surfaces over which the ore passes a steep inclination, and by excluding all flat ledges. This is, perhaps, all that need be stated in order to make the general construction and operation of the machine clear.

Having designed a machine according to a geometrical principle, it is interesting to all engineers to know how the practical results correspond to the theory when the machine is put to the test. As to this, the machine will now speak for itself. We will subject it to one of the most severe trials that could be applied to a sampling machine, and the precision of the work will be tested by making a mechanical analysis of the samples obtained before you. The material I propose to use is a mixture of three substances of different size, shape, and specific gravity. We will first weigh each one singly before combining them, so that we may know the exact composition of the mixture. Then, without any preliminary attempt to mix the three materials, the bulk will be passed through the machine, and two samples will be taken, each of 10 per cent. Their weights will be compared with one another, and with 10 per cent. of the bulk. Each of the samples will be then separated into its constituents by sifting, and the percentage of each

constituent ascertained by weighing, and compared with the percentage of each constituent in the bulk.*

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* *Description of the Experiments.*—The three materials which had been selected for the weighing were wheat, peas, and sand—for the two reasons that such materials are practically unmixable, and therefore most difficult to sample in the ordinary way; but, although difficult to sample such a mixture, it is very easy to test the accuracy of the sampling by sifting out and weighing the constituents. A mixture was then prepared by weighing out

12·5	lb.	of wheat
7·5	„	peas
5·0	„	sand
25·0		

As each lot was weighed it was thrown into the hopper of the machine, the result being that the materials were in a more or less stratified form, or, in other words, they were as badly mixed as was possible. When the three lots were weighed out the machine was turned, and the material allowed to fall into it by drawing out a slide. In a few seconds the work was done, and two samples were obtained which each weighed 2·5 lb., or 10 % of the bulk, thus proving the precision of the machine in taking portions of definite percentage. Then, to prove that the portions were true samples, one of them was sifted so as to separate the three constituents. These were then weighed, and gave the following results:—

1·25	lb.	of wheat
·75	„	peas
·50	„	sand
2·50		

A small size machine weighing about 7 lbs. has been made of aluminium. This may be found very useful for prospecting purposes. Of course, the prospector, in cutting down his kit to the lowest possible weight, is more particularly concerned about what he can dispense with, rather than what he can do with. And the best man in any emergency will always be the one who can accomplish the most with the fewest number of appliances; but all the same, the superior accuracy and precision of machinery will always tell in favour of its adoption.

The operation of the machine is not confined to any particular class of materials. Without any alteration, it may deal with solids of various sizes from, say, six inches down to flour, and with but slight modification the machine will operate with equal efficiency upon liquids and pulp. This brings to notice a very important matter—viz., the reliable sampling of tailings from a gold stamp mill, to ascertain the amount of loss of gold, and the value of the tailings, with a view to their subsequent treatment or disposal. To obtain trustworthy indications by hand needs very regular attendance at stated intervals, say every hour, for the purpose of taking a portion from the stream. It is important that an equal portion be taken each time, and when it is also remembered that the value of the tails at different points across the stream may vary, the difficulties are seen to be considerable. The difficulty of properly sampling tailings was pointed out to the author by one of the members of this Institution, and a machine has been made upon exactly the same principle as the one shown for solids, but with necessary modifications of detail. It is found that the principle of a rotating annular stream applies very well to a mixture of solid and liquid such as battery pulp, and as the solid is all finely crushed, it is possible to

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On releasing a plug at the base of the bin, the ore descended into the sampler, which took two samples of 5 %, and the rest of the ore then fell into one of the empty trucks beneath. The final demonstration was with the laboratory dividing machine, which was used to split up a sample into six equal and exactly similar parts. This operation was successfully completed in less than six seconds.

take a sample of only 1 per cent. at one operation. The tailings from 5 head of stamps can be easily passed through one sampler as they flow away, a true sample is retained and the bulk passes on. Suppose that in 24 hours the five stamps crushed 25,000 pounds, or about 11 tons, and that 10,000 gallons of water are used. A 1 per cent. sample of this would consist of 250 lbs. of quartz and 100 gallons of water. Now this would be too large a sample to deal with, so it is reduced by taking a 1 per cent. of it on a smaller sampler, forming really a part of the other one, so that the operation would go on continuously without any intermediate handling. As the net result of the day's work, we should have 2½ lbs. of quartz in a gallon of water, quite a manageable quantity, which could be collected in a locked vessel if necessary. As representing this sample, I have a gallon of water in a vessel, and 2½ lbs. of dried tailings in a sealed bottle, which can be labelled and kept for future examination. In this way, an automatic daily record of the value of the tailings could be kept, somewhat corresponding to our daily barometer charts, and as the personal element does not come into the sample, every confidence could be placed in the accuracy of the result.

The adoption of such an automatic arrangement of sampling would throw full light upon the condition of things at the tail end of the battery, enabling the exact value of the tails to be ascertained, and if a proper system

exceptions where hand selection is indispensable. If it was possible to put the whole of a lode through the machine, I would recommend it to be done, but failing this, a large sample—and the larger the better—should be carefully selected by hand, then crushed and passed through the machine. Speaking generally, this system can be applied to all materials that will run, or that can be made to run from a hopper, whether solid or liquid. The above examples are cited as indicating rather than exhausting the usefulness of the machine. Before bringing this somewhat rambling paper to an end, I would like to refer to the President's admirable address, in which reference was made to the great progress which has been achieved in the metallurgical industries of America, and to the increasing keenness of the competition between the Old and the New World.

Permit me, in conclusion, to emphasise one reason for this state of things. In America the mechanical spirit is strong and vigorous—that is, the spirit which prompts the adaptation and adoption of machinery wherever possible in the place of hand labour, and the attitude of Americans towards the investigation and adoption of new departures and improvements is generally described by the expression, "Why shouldn't we," whereas with us the general attitude is, "Why should we": why should we, until compelled, revise our methods, and disturb a condition of things which has been as it is for so many years. It is thus about sampling. The present methods are the outcome of many years of practice, and they embody the results of many disputes between buyers and sellers. It is, therefore, only natural that there should be hesitation in abandoning the old way in favour of a new one. But when it can be conclusively shown that there is a much better and cheaper way, further delay in the adoption of the superior system can be only accounted for by prejudice. However, as prejudice is an expensive indulgence, sooner or later the pocket is touched, and then prejudice yields before the irresistible advance of true practical science.

DISCUSSION.

Mr. C. Algernon Moreing: As far as I am aware, up to the present time no machine so perfect has ever been brought under the notice of mining engineers, although machines with the same object have been in use in America. The results of the experiments carried out this evening upon the various mixtures have been simply startling; indeed, it seems to me that in the future no gold mill can be considered complete without this machine. One of the most essential features in treating gold ores by milling is certainly to have correct samples of the ore as it goes into the mill, and also correct samples of the tailings as they go away from the mill, in order to see what amount of the contents is being saved, and where the loss, if any, is taking place. Up to the present that has been not only a laborious operation, but most inefficient and anything but scientific. Most mill men are not particularly desirous of having good samples of the tailings; in fact, they take care not to have them. With one of Mr. Clarkson's machines we should have an absolute check upon the men, and I do not think, as I said

is a pretty difficult business when you have a large mine and a hard vein of quartz; but after you have got your large sample, breaking it down and then quartering it up is one of the most laborious and most unsatisfactory operations that any man can undertake, because it is so difficult to get an accurate sample of the sample you have taken in the mine. I am sure it will be a great boon to inspecting engineers to have a little machine of that sort with them. If ever I go in for inspecting work again—and I sincerely hope I never shall—I shall certainly take one of them about with me. I cannot say that it will be of much use to the prospector. He is not a man who goes in for scientific sorting, and I think when Mr. Clarkson refers to the prospector he has probably in his eye the inspecting mining engineer, who is a much more ill-used individual. Of course, beyond gold mining and mine inspection there is an immense use for the machine in sampling all kinds of ores that are sold by sample, such as manganese ores, silver lead ores, and many others.

Mr. John McKillop said he had had much experience in sampling, and he wished to tender his thanks to Mr. Clarkson for having made easier a most difficult work. Some four years ago he had devised, with the assistance of a Chinese tinsmith, a machine which acted in something the same way. He had not had sufficient ingenuity to give a rotary motion to the machine, and consequently it was not quite so satisfactory a production as Mr. Clarkson's appeared to be. The greatest difficulty was that the stuff had to be reduced to a certain size—that ultimately adopted being one-sixth of an inch mesh—before accurate results could be obtained. Until the stuff was so reduced to size there were considerable variations in the samples. Another difficulty was occasioned by the fact that most of the ores reached the works damp and the machine became clogged, thus necessitating recourse to the undesirable practice of drying them. He should very much like to see Mr. Clarkson's machine adopted by the tin people down in Cornwall. Were the pulp leaving the batteries passed through such a machine, as well as the tailings, he

thought it would go a long way towards obviating the frightful waste that went on there. When he was last down there he had found that one-sixth, approximately, of tin put out in Cornwall was taken away by the stream and lost to the shareholders. Tin mining in England was now in quite a sufficiently sick state to warrant the introduction of some such check.

Mr. Thomas Rickard said the Cornishman would be extremely obliged to the last speaker for his commiseration, but unless he were mistaken Mr. McKillop had not informed himself as to how sampling was done in Cornwall, and that was precisely the complaint he had to make against the paper. Instead of being a paper on sampling, it really gave a lot of padding about things which might advantageously have been altogether left out. The author had brought up the old matter of gold being found in copper that by some means eluded the seller, and about mica and pyrites being taken for gold. These were the things he used to hear about in his childhood, and had long since been supposed to have been relegated to oblivion. Personally, he did not think the Cornishman was so far behind the age in his sampling, on the contrary there was no operation connected with mining anywhere which reflected more credit miner than the manner in which he dealt with upon the large parcels of copper ore. Mr. Clarkson might certainly have dwelt with advantage upon the extremely careful

there for the purpose of being critical—that the paper too much exalted the mechanical arrangements adopted in America. So far as he knew, the engineers of this country were quite as much alive to the necessity of adopting mechanical and automatic arrangements as the engineers in America. He thought the author had not given himself the same trouble to find out what was being done in England as what had been done in America. It was, perhaps, unfortunate for the invention that Mr. Clarkson had spoken of it as being recommended by the smelters at Swansea. As to the prospector, the machine would obviously be of no use to him whatever. He would never put one into his kit. There was a good deal of interest, however, attaching to the question of how the machine was applied in taking a sample of effluent tailings. Perhaps Mr. Clarkson would explain whether there was any intervention of hand labour in the operation, or whether it was carried out under lock and key so that it could not be meddled with, and whether it would register its own results. Having said so much in criticism of the author's paper, he would add that his excellent machine would get plenty of commendation, and he should regret it if he were not as forward as anyone in adding his word of praise and satisfaction at the manner in which Mr. Clarkson has overcome the difficulties incidental to the production of an effective mixer—such as he held the machine to be.

Mr. James Mactear: I scarcely agree with the last speaker, who speaks of this machine as not being a sampler but a mixer. Now, I do not see how any machine can be a sampler without being a mixer. The whole gist of a sampler is the complete mixing and the taking out of a proportionate part of that mixture. We have had machines of this kind tried in America, more particularly where cones have been used with slots in them of varying sizes so as to take out varying proportions. As far back as 1884 I saw these machines at work at Denver, and they worked apparently very well; but there was always the difficulty that there might be a particular stream going down between the two openings of the cone, and you did not

therefore get an absolutely accurate sample. To show the absolute necessity of accurate sampling, I may state that at one time I was using very large quantities of pyrites, and I had under my charge three works. We bought the copper, and after having burnt the pyrites we re-sold the cinders containing the copper. Now, owing to the fact that the dry assay on which the copper is sold is affected to a considerable extent by the richness of the ore, the result of such an operation as buying green ore and selling the burnt ore should always leave a very fair profit on the transaction. That profit, we calculated, should have been between 5s. and 6s. per ton at that time, and for some years at one of the works the profit ran from 5s. and 6s. a ton, and even up as high as 7s. 6d. a ton one year. Another of the works, however, not only did not make that profit, but for two years running had a considerable loss. I investigated the matter thoroughly, and found it to be entirely due to the question of sampling, and you may imagine what a serious matter it was when I tell you that in those works there were over 1,800 tons of pyrites a month on which a profit should certainly have been made of at least 4s. a ton. Of course, no manager can constantly be on the spot to see samples taken of 100 tons a day, but if a whole waggon load could be tipped through a machine like this, and aliquot parts taken out, as apparently can be done, then such risks of loss should be very much reduced.

[redacted]

of the tin raised from the mines in Cornwall goes down the rivers and is afterwards recovered from them is no evidence whatever of bad work at the mines. The fact is that the one-sixth afterwards recovered, like the five-sixths originally recovered, costs in the first instance very nearly as much to get as it is worth—a fact which is not to be overlooked. If the one-sixth were subsequently got for nothing, there would be room for commiserating the poor shareholders. I should like Mr. McKillop and other gentlemen who seem to think that this wanton waste of tin is going on in Cornwall to consider such a problem as this. I could take them to one place in Cornwall where, during the whole of last year, and at the present time, tin ore is being quarried and trammed and crushed and dressed for less than 1s. a ton, all told. Now, if Mr. McKillop can tell me of any place in the world where such work is done at a cheaper rate, I shall be glad to know of it.

Mr. J. B. Hannay: The motion imparted to materials on the surface of rotating cones (and it is immaterial whether the cone or the material rotates) is being used more and more as a method of obtaining uniformity of mixture. Although the machine exhibited is not a mixer, as it does not mix the upper and lower layers, yet it is more than a divider, because if some material comes down one side of the rotating hopper, it is distributed by the fan-like motion so as to spread it equally round the circle. Hence it seems to be a distributor and divider, and to do the work with great accuracy. There is a wide field for its use in chemical works for sampling salts, &c.

Mr. McKillop said that when he referred to the use of the machine in Cornwall, he was not speaking of sampling the stuff as it came from the mine, but of sampling the pulp as it came from the stamps.

Mr. A. C. Claudet said he would like to have one point cleared up; whether the machine was a mixer or divider. It seemed to him that it was essentially only a divider or sampler, and not a mixer.

Mr. A. G. Charleton: As far as I know there are two classes of samplers in use, one in which the whole stream

is taken part of the time, and the other in which part of the stream is taken the whole time. It is a very important point to determine to which of these two classes this particular machine belongs, because I have heard it stated by American mining engineers that, so far as their experience goes, the best results have been obtained by machines which took the whole material part of the time. You have examples of such in the Brunton and Bridgman machines. The whole stream is deflected from its course and falls into a separate receptacle, by which a 2 per cent. or a 4½ per cent. sample, as the case may be, is got. The objection raised to machines which divide the stream during the whole time is that unless the ore is perfectly dry, or if it contained chips of wood or pieces of rag, the work is not always as satisfactory as it would be when the ore is of an even grade. I think the great use of mechanical samplers—and they have been very largely introduced indeed in the West—has been in works where ore is sent to a third party to be sampled for the buyer and seller. His returns are taken as the basis on which to determine the value of the sample and to come to an agreement. As far as the accuracy of the sampling itself goes, I believe you could get just as good results with hand sampling as with machine sampling, provided the hand sampling is carried out on a proper system—that is to say, you gradually reduce the size of the pieces of ore and

should like to have some further information. It seems to me that the question of size must to a certain extent enter into the matter. You cannot have material greatly varying in size. Then there is the question of moisture. There is no doubt that the machine, being a revolving one, would pass through it material in a condition which would not pass through a stationary machine.

Mr. Thomas Clarkson: In describing such a machine there is necessarily a great deal which one has to leave unsaid, and, as it has been described in one or two other papers, I have been most careful to eliminate every portion that has been previously spoken of. There appears to be a misapprehension as to what the machine is. It has been declared by Mr. Rickard, and one or two other gentlemen, that a machine, in order to be a sampler, must necessarily be an efficient mixer. This appears to be a general notion, but it is an incorrect one. This machine is not a mixer, as it has been so emphatically declared to be; it is a sampler—that is, it obtains a sample by taking an aliquot portion from whatever kind of materials may happen to be running through the machine at any instant, and it is perfectly immaterial whether the stuff fed into the machine has been previously mixed or not. Of course, thorough mixing is a *sine quâ non* in hand sampling, but I do not require any mixing with this machine, and the material is delivered from the machine in very much the same condition as when fed in. Whether the first part that comes through is all fine, or all coarse, or of an intermediate character, it does not matter, because the machine takes the same definite percentage of whatever kind of material happens to be delivered by the hopper at any instant; and clearly the machine does not act as a mixer, for if all the rich material were at the bottom of the hopper it would rush through first, and not mix with the rest. Whatever is at the bottom of the hopper will be at the bottom of the receptacle, so that no mixing takes place. I dispense with mixing entirely, but I am very glad that Mr. Claudet referred to that point, because it is not clear to many that a machine to be a sampler need not be a mixer. I know it is

a very common idea. I am afraid I am not qualified to speak of the systems in vogue in Cornwall. All this machine will do is to facilitate the exact measurement of the value of all the materials. It will thus show what is going to waste, it will show you the quality of the material going into your stamps or concentrators, and it will show you what is going out as tailings. It does not actually save more; but it shows where the work can be improved, by revealing where the loss is taking place, and to know your enemy is certainly half the battle. With reference to the Americans, I think everybody will grant that they are certainly ahead of us in mechanical contrivances. This is extremely patent to anybody who goes over there and sees how mechanical ingenuity is brought into almost everything. The Americans, however, have many advantages over us. They started with the advantages of European experience, and they have no old prejudices to overcome, such as are so strong on this side. Still, although they have been using sampling machines for some time, one or two of which have been more particularly mentioned by Mr. Charleton, these are not so perfect as they should be. There are two systems of machines — one for taking the whole stream of material for a portion of time, and the other for taking part of the stream the whole time; the one intermittent, the other continuous. The best American machines are of the former class but

ments I have made give a conclusive answer to that question. It is perfectly immaterial whether the stuff going into the machine is of the finest or of the maximum size that the machine will take. It is a peculiar feature of the annular rotating stream that although we may not know how the material is distributed, yet if a true segment of the stream is taken we can be certain that it contains average portions of all sizes of material. Of course, this machine will not make men more conscientious than they are inclined to be, but as I tried to show, it will put it beyond the power of men to interfere with the operation of sampling. The whole operations, as I explained in the case of the effluent tailings, can be done under lock and key. Professor Huntington has raised a point as to the size of material and the degree of moisture. The size of material that can be passed through a machine has been largely a matter of experiment. At first, I calculated that the diameter of the nozzle of the hopper ought to be about four times the maximum dimension of the largest piece, but I have found from actual tests that nothing like that is required. As a fact, the nozzle of the No. 1 machine exhibited is 1¼ inch in diameter, and has easily passed pieces 1 inch by ¾ inch, and of course there would be other sizes smaller than that passing at the same time. That we need not trouble about in the smallest degree. There is a certain maximum size of ore for each size of machine—for example, a No. 5 will take pieces up to 3 inches diameter, and it will, of course, also sample anything finer than this down to the finest dust. All that a man needs to do is to turn the handle of the machine, and so long as a fair rotation is given, it does not matter. As to moisture, tests have been made purposely with damp material—some of the dampest, finest, and most refractory that could be got, carrying from 15 to 16 per cent. of moisture—and we have succeeded in getting it through the machine. It has been necessary, however, to use a fixed steel blade passing down the side of the rotary hopper, so as to stir up the material, otherwise it blocks into a compact cone and will not run. Generally, I may say the machine will easily sample anything that will run or can be

made to run from the hopper; and if necessary not only can the sample boxes be locked, but the whole machine can be made perfectly dust tight, as may be desirable for some purposes.

SEVENTH ORDINARY MEETING.

The Seventh (and concluding) Ordinary Meeting of the Third Session was held on Wednesday evening, 20th June 1894, in the Lecture Theatre of the Geological Museum, Jermyn Street, S.W., at 8 o'clock.

Professor A. K. Huntington (President) occupied the chair.

The Secretary having read the minutes of the last meeting, which were duly confirmed, and having announced the names of those gentlemen recently elected to the Institution,

Mr. C. Algernon Moreing then read the following paper :—

THE MANGANESE MINES OF LAS CABESSES, PYRENEES, FRANCE.

By C. Algernon Moreing (*Member*).

The mines yielding carbonate of manganese at Las Cabesses, in France, are of special interest to mining engineers, as they are the only mines of this mineral worked on a large commercial scale—and, indeed, the deposits are the only ones of large size as yet discovered; moreover, having so recently been opened up, their existence and history is unknown to many members of our profession.

Till quite a recent year (1890) the only ore of manganese known to commerce was the black oxide, the main sources of supply being Chili and the Caucasus. In 1880 a deposit of black oxide of manganese was discovered at Las Cabesses and mined in a small way by open workings. From 1881 to 1888 about 5,000 tons of oxide, containing from 45 to

In the following remarks, the Author will endeavour to give a succinct description of this important mineral deposit, which is destined to play a considerable part in the supply of manganese ore to the iron furnaces of Europe and America.

The mines are situated 15 kilometres (9·3 miles) from St. Girons, in the Department of Ariège. They are held under a concession from the French Government, the only obligation being the payment to the Government of 5 per cent. of the net profits of the undertaking, and compensation for surface damage effected by the mining works. A plan of the concession, which covers an area of seven square kilometres (1,729·05 acres), is shown (Plate VII.). The district, being in the Pyrenees, is of course exceedingly mountainous. The concession itself is situated on the side of a mountain which rises very precipitously to the north from the Valley of the Nert. It is interesting to note that "Rivère Nert" means in the patois of this part of France "black river," and has evidently taken its name from the black oxide of manganese met with in its bed. The geology of the district is readily grasped and presents no features of complication or difficulty to be accounted for, except the deposit of manganese ore. The strata strike east and west and have a regular dip to the north of about 72°. The lower beds consist of Silurian schists, overlaid conformably by calc schists, which pass, imperceptibly in places, into mixed strata of "griotte" (marble, with red and brown spots) and limestone, these being succeeded by a bed of pure griotte, which is overlaid by the shales of the coal measures, as shown on the section.

The calc schist is distinguished as of Lower Devonian, the mixed griotte and limestone as of Middle Devonian, and the pure griotte as of Upper Devonian age. Specimens of the various rocks are submitted.

The manganese ores are found in the middle beds, or mixture of griotte and limestone. The deposit at Las Cabesses, so far as at present developed, is a mass having a length of about 60 metres (196·8 feet), and a width of about

50 metres (164 feet), and is now opened to a depth of 70 metres (229·6 feet). An interesting problem for mining engineers is to formulate a reliable theory as to the formation of this deposit, as an accurate knowledge of the mode of its deposition would enable the researches to be continued in an intelligent and scientific manner. Other points of interest to practical men are: (1) How to work a deposit of this character so as to extract the ore with the least waste; at the smallest expense; and without danger to the workmen. (2) How to handle and treat the ore for market. It will probably, in the first instance, be advantageous to quote a description by M. Mettrier, one of the State mining engineers of France, who has had exceptional opportunities of studying the deposit from its first discovery: "Il occupe le fond d'une vaste cuvette, large de près de 200 mètres, et y est recouvert par une épaisseur de 8 à 10 mètres d'argiles et de terres meubles; sous ces terres, la surface du carbonate, ainsi que les parois des fractures qui descendent dans le gîte, ont été transformées en oxyde friable, qui forme en divers points des couches peu puissantes, exploitées de 1881 à 1890, surtout à ciel ouvert, et en quelques points au moyen de puits et de descenderies; la production en ce laps de temps a été de 5,000 tonnes environ de bioxyde à 40 ou 45 % de manganèse. Les parties oxydées sont aujourd'hui à peu près épuisées, et l'amas carbonaté est exploité au

C'est aux niveaux de 14 et 18 mètres que le gîte présente ses plus grandes dimensions, sous réserve bien entendu des découvertes que pourra amener l'exploration des étages inférieurs. À 18 mètres, notamment, où l'allure est plus régulière qu'à 14 mètres, en raison du plus grand éloignement de la surface, le minerai forme deux amas allongés de l'Est à l'Ouest, chacun présentant en moyenne 50 mètres de longueur et 12 à 15 mètres de largeur, avec une cloison séparatrice de 6 à 8 mètres d'épaisseur. La colonne septentrionale est inclinée d'environ 45° vers le Sud, tandis que la colonne méridionale, limitée au Sud par une grande faille verticale, est elle-même sensiblement verticale, en sorte que ces deux colonnes n'en forment plus qu'une au niveau de 25 mètres; en outre, leurs axes sont inclinés vers l'Ouest, de façon qu'en profondeur il faut aller chercher le prolongement du gîte toujours plus avant dans cette direction. On explore aussi, aux niveaux supérieurs, diverses veines minérales qui se dirigent vers l'Ouest et le Nord-Ouest et qui peuvent conduire à d'intéressantes découvertes, soit le long de la grande faille Sud, soit vers le contact des schistes houillers qui surmontent au Nord l'étage griottique. La puissance de cet étage est d'une centaine de mètres, et il est compris entre les schistes houillers et les calschistes dévoniens, schistes et calschistes plongeant fortement vers le Nord en parfaite concordance. La stratification des griottes intermédiaires est au contraire indécise; elles forment fréquemment, à la colonne minérale le plus au Nord, un toit ou un mur très nets, plongeant à l'Ouest ou au Sud-Ouest, avec intercalation au contact du minerai de bancs de schistes noirs qui attestent la formation sédimentaire du gîte; mais souvent aussi le carbonate passe au calcaire marbre par des transitions insensibles, ou bien il en est séparé par des fractures incurvées qui délimitent plus ou moins complètement des amandes de toutes formes et de toutes dimensions. Les bancs de schistes entourent aussi parfois ces amandes, et l'ensemble de ces caractères, montre qu'on se trouve en présence d'une formation fracturée et bouleversée en tous sens, ne constituant plus aujourd'hui

qu'une sorte de magma dont les éléments présentent l'allure la plus irrégulière." *

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* *Translation.*—It occupies the bottom of a large basin about 200 metres in diameter and is covered by a thickness of eight to ten metres of soil and loose earth. Beneath this the surface of the carbonate deposit, and also the sides of any fissure-joints in the deposit, have been changed into friable oxide which forms in some places thick beds. . . . The deposit of carbonate is worked by means of eight levels at 8, 14, 18, 25, 33, 39, 44 and 52 metres below the surface of the basin with an adit level 203 metres long which strikes the deposit at the 52 metre level. The irregular nature of the deposit, and the veins and "horses" of country rock which it contains, causes the size and arrangement of these levels to be very irregular. The deposit, as far as now worked, is of the largest dimensions between the 14 and 18 metre levels, but of course exploration at lower levels may show other developments. At the 18 metre level especially, where the form is more regular than at the 14 metre level, in consequence of its greater depth below the surface, the mineral forms two elongated masses each about 50 metres in length from east to west, and 12 to 15 metres in width, with a "horse" of marble between them 6 to 8 metres in thickness. The northern mass or column is inclined about 45 degrees towards the south whilst the southern column, which is cut off on the south by a large vertical fault, is itself almost vertical, so that the two columns join together and form one only at the 25 metre level. Their axes also dip towards the west, so that in depth the continuation of the deposit must always be looked for in this direction. There are also in the upper levels several veins of mineral which run west and north-west, and which may lead to interesting discoveries either along the large fault on the south, or the contact of the coal schists which

Mr. Edward Hooper, M.Inst.M.M., who has also had an opportunity of studying the mines, has furnished the Author with the following description and theory of the formation of the deposit :—

From the cursory examination that I was able to make of the geological features of the deposit of Manganese ore at "Las Cabesses," I am inclined to support the theory of "replacement," aided by lateral secretion.

In this process of "replacement" it is not supposed that any great cavity has previously existed, but that there has been a metasomatic deposition of Manganese by the agency of ore-bearing solutions, the general structure of the calcareous rocks being preserved, while their substance was gradually replaced by the Manganese.

J. F. Kemp, in his treatise on "The Ore-Deposits of the United States," aptly describes this process of replacement as "a circulation or percolation of ore-bearing solutions which exchange their metallic contents, molecule by molecule, for the substance of the rock mass."

One of the characteristic features of this deposit is the absence of any well-defined boundaries; that is, the ore generally passes insensibly into barren material, except perhaps where a dividing seam or selvage of talcose clay has probably acted as a barrier to the waters carrying the ore in solution. It may also be noted within the limits of the deposit that in many cases the rock is to some extent *impregnated*, but not altogether *replaced*, by the Manganese ore.

It has been satisfactorily proved that the purplish calc schists —which constitute the lower beds of the Devonian System and underlie this deposit to the South—contain small quantities of Manganese, and doubtless owe their peculiar colour to the presence of that mineral.

The "Simon" Adit, which was driven in a northerly direction through these calc schists, intersected a large open fissure at the southern boundary of the ore-body, from which a considerable amount of water still continues to flow; and there are also other evidences of existing water-channels, which, however, appear to be confined to the southern part, or what may be termed the foot-wall of the deposit.

In addition to these and other natural springs, the numerous basin-like depressions of the surface, along the strike of the limestone and "griottique" beds of the Devonian System, are sufficient evidences of the presence of underground streams or water-courses. The most noticeable of these depressions is that forming the mouth or outlet of the "Las Cabesses" deposit. The

passage of the ore-bearing solutions at this particular point has undoubtedly been assisted by the presence of fractures in the original rock.

Mr. Hooper's theory will probably account satisfactorily for all the peculiarities of the formation, but it furnishes no indication as to the extent of the deposit in depth or longitudinally. There is only one system of exploration open, and that is to follow the mineral wherever it goes. The mines up to the present have been worked by French engineers, and a series of levels have been driven at depths of 8, 14, 18, 22, 25, 29, 33, 39, 45, 52, and 57 metres, shafts being sunk from the surface at different points to the 25 metre and 33 metre levels, and an adit 203 metres in length driven to intersect the deposit at the 52 metre level. These levels were connected by numerous irregular holes in the nature of winding staircases. and there is one continuous connexion from the 52 to the 33 metre level for the removal of the ore. The levels themselves are most irregular in form, as will be seen from the accompanying plans. It will also be noticed that these levels have been driven so close together that there is in most cases only 4 metres distance between the roof of one and the sole of another, and sometimes even less. No stoping was attempted, all the ore being extracted from drifts in an expensive and wasteful manner. Moreover, being thus honeycombed, the mine was left in a very awkward condition to deal with in the future.

[redacted]

slices or layers, each having a vertical thickness of about 20 metres. At the bottom of each layer about 4 metres of solid ground is to be left intact to serve as a floor, which will support the debris as it is filled in to take the place of mineral stoped out. The 33 metre level has been selected as the foundation or floor for the extraction of the first of these horizontal slices of ground. A series of drifts, more or less parallel, with branches therefrom, will divide the deposit into sections convenient for extracting and handling the mineral. These drifts will be protected on either side by a substantial stone wall (0·90 metre wide), except where pillars of barren rock happen to form one or both of the sides, in which case the walling will not be necessary. The width of these drifts, or the space between the above-mentioned walls, will be 1m. 40 rising to a height of 2m. 10, when they will be covered with stout timbers, having a minimum diameter of 0·10 metre at the smallest end. During the construction of these drifts all open spaces between the sole and roof of the 33 metre level will be gradually filled up with debris; and as soon as this is completed the extraction of the ground above the 33 metre level will be proceeded with in the ordinary way. Chutes will be carried up, about 8 metres apart, along these drifts for the delivery of the ore from the various workings above.

There are two shafts which can be made use of. These are shafts Nos. 5 and 6, and they will be utilised for the purpose of bringing down from the surface such debris for "filling-in," or rock for building walls, &c., as may be required from time to time. Now, as the upper portion of this deposit is more or less friable, being composed chiefly of binoxide of manganese, it is proposed to commence at the surface and work downwards, stripping off the deposit to its limits in horizontal layers. In this way the necessary debris for the filling up of the interior of the mine will be obtained, while the load of loose material above the 33 metre level will be reduced, and the risk of any accident by subsidence minimised.

The production of carbonate of manganese at present is about 100 tons per day, the ore being carried from the

mouth of the adit to the roasting kilns, situated in the valley of the Nert, by a cable tramway of the Otto-Pohlig system, 1,600 metres in length, and having a fall of 238 metres, or one in 6·72. The ore is roasted to drive off the carbonic acid in kilns similar to those used for calcining carbonate of iron. Plate XI. shows the type of kiln employed, which consists simply of a cylindrical sheet-iron shell, open at both ends, lined with fire bricks, and resting on iron standards rather less than 1 metre in height. There are eight of these kilns at present in use. Although an extremely simple operation, the roasting requires greater skill and care than does that of carbonate of iron, as the ore has a tendency to slag. The furnaces are so erected that they can easily be got at from any side, and so that the air can have free access to them. The details of construction may be seen by reference to the drawing. They are 5 metres high, and 3 metres wide at the mouth. They each have a capacity of 45 tons of raw material, but are charged about 50 to 60 centimetres higher than the mouth of the kiln, the nominal capacity being thus increased by from 2 to 3 tons. Under the kilns there is room for 10 tons of calcined mineral, so that, from the ground to the top, they hold about 55 tons of ore each.

The way in which the kilns are charged is as follows :—

About ten tons of calcined mineral, or raw material if

of the kiln; and on the third day the material is drawn down a little in order to bring air into the kiln, and prevent the mineral becoming slagged into lumps. After having thus begun to draw the ore and fuel down, the kilns are filled to the top with layers of coke and ore alternately. When charging the kilns for the first time, only big pieces of ore are chosen, no small stuff being admitted with it, so that the draught may not be interfered with, as it is essential to have a good circulation of air. Once the kilns are in operation, the layers may consist alternately of large pieces and small stuff. The whole of these layers must be nicely arranged, in order to keep a constant draught. The third day after the kilns are filled, drawing is going on; that is to say, the bringing down of the ore at regular intervals, and fresh ore is added on top. When the kiln is in full operation—say after the sixth day—there are three drawings a day and three at night, every four hours regularly; and by this means from 8 to 10 tons of calcined ore are drawn out of each kiln in 24 hours. When the kilns are properly started, the working is very easy; and it only requires care and drawing and filling at regular intervals. The ore is broken for the furnace to pieces not larger than say from 4 to 5 inches square, and the smallest stuff put in is of the size of walnuts. The proportion of the different sizes of material introduced are as follows: fourteen wheelbarrows of large ore, with eight wheelbarrows of small stuff, to two wheelbarrows of coke. The layers follow each other regularly, and are arranged so that in the centre of the kilns, as well as at the sides, there is always big stuff, whilst the small ore occupies the intermediate space; the large pieces of ore allow the air to circulate, and form a kind of chimney, while the coke is spread out as regularly as possible, and forms a thin cover over each layer of mineral. The proportion of coke used is from 2½ to 3 per cent., by weight, of the raw mineral.

The loss in weight of the ore for the year 1893 was 32 per cent. on the average, which may be regarded as a very good result, as the mineral contains from 33 to 36 per cent. of carbonic acid.

The ore as broken in the mine contains a considerable mixture of marble or barren matter, and consequently a system of cobbing and hand-picking, both before and after calcination, has to be resorted to, in order to remove the waste which would otherwise seriously affect the value of the calcined ore in the market.

The picking of the ore before calcination is at present insufficiently done, owing chiefly to the lack of water and mechanical appliances.

Most of the ore of "walnut" size is not assorted at all but goes direct to the second class kiln. All large stuff is tipped at present on to a platform and washed by hand pump or syringe. When the ore is of good quality, the sterile rock and second class ore are picked out, the remainder going direct to the first class kilns. When of lower grade, the barren rock and first class ore are picked out by hand, the rest being second class ore. All mineral having a preponderance of "smalls" is tipped on to an iron grizzly (having spaces of 5 centimetres between bars), and the small stuff which passes between the bars is then classified by a trommel (holes 1·80 centimetre square), worked by hand, from which the "coarse" goes direct to the second class kilns without assorting, and the "fine" is loaded direct into waggons and sold as "raw product." The larger pieces which fail to pass through the grizzly are

purposes into first class calcined, second class calcined, and "raw product." The average contents are :—

1st Class ...	50 to 54	per cent.	metallic manganese.
2nd Class ...	43 to 45	"	"
Raw Product	33 to 36	"	"

Plans are now under consideration whereby the ore, before calcination, will be handled more automatically, and it is expected that with better facilities for classifying the ore, the calcined product drawn from the first class kilns will require but little assorting. There would be a larger proportion of the first class ore than at present.

On account of the average size of the ore charged into the kilns as first class being larger than that which goes to the second class, the calcination thereof is more rapid, which can be easily understood, and, moreover, less coke is used with the first class than with the second class ore.

In conclusion, the following tables, compiled from trustworthy sources, are submitted, showing the composition of various Manganese ores and the production thereof.

Table I. gives analyses of Las Cabesses ore—not of specimens, but the average of sales of many thousands of tons.

Table II. shows the composition of Manganese ores from various other parts of the world.

Table III. shows the Manganese production of the United Kingdom; a poor record, exhibiting a great falling off in 1893 from even the small production in 1892.

Table IV. shows the production in Chili since 1885.

Table V. shows the production of the World since 1888.

Table VI. shows the weight of ore imported into the United Kingdom during the years 1891—93.

These figures indicate the immense increase in the consumption of Manganese in the last few years, a consumption ever augmenting; and it must be satisfactory to the great consumers to know that there is such a magnificent deposit of the purest and richest ores at the easily accessible mines of Las Cabesses in the Pyrenees.

TABLE I.

Analyses of Ore as sold from Las Cabesses.

	Manganese. Per cent.	Silica. Per cent.	Phosphorus. Per cent.
Black Oxide (from surface)	48 to 55	5 to 10	0.048
Carbonate (raw)	40	5.9	0.048
Carbonate (calcined)	54.3	9.3	0.041

TABLE II.

Analyses of various other Manganese Ores of Commerce.

Country whence derived.	Composition. Manganese. Per cent.	Iron. Per cent.	Silica. Per cent.	Ph'sph'rus. Per cent.	Moisture Per cent.
Wales	30	2	24	·02	3
Sweden	47	1	16	·01	2
Australia	48	6	3	·30	3
India	49	10	3	·13	1
Cuba	50	1	8	·08	2
Chili (Coquimbo)	50	1	5	·05	1
Do. (Carrizal)	50	1	10	·09	1
Spain (Huelva)	50	2	10	·10	3
Portugal	51	3	8	·11	3
Japan	52	2	9	·10	7
Caucasus	52	2	8	·18	6
New Zealand	53	2	8	·07	6

TABLE III.

Manganese Ore Production of the United Kingdom.

Country	Statute Tons	Value at the Mine

TABLE IV.

Chilian Production of Manganese Ore.

Year.	Coquimbo Long Tons.	Carrizal. Tons.	Total. Tons.
1885	4,041	—	4,041
1886	23,701	227	23,928
1887	38,234	9,287	47,521
1888	12,132	6,581	18,713
1889	9,145	19,538	28,683
1890	23,409	24,577	47,986
1891	16,462	18,000	34,462
1892	17,509	18,457	35,966
1893	13,815	27,000	40,815
Totals	158,448	123,667	282,115

TABLE V.

Manganese Ore Production of the World.

	1888. Lng. Tns.	1889. Tons.	1890. Tons.	1891. Tons.	1892. Tons.	1893. Tons.
Russia (Caucasus)	48,653	60,000	120,928	84,607	125,724	134,518
United States	29,198	24,197	16,000	11,000	6,000	4,000
Chili	18,713	28,683	47,986	34,462	35,966	40,815
France (1886)	7,676	—	583	2,378	10,079	24,466
Sweden	6,089	18,000	5,634	5,608	4,001	1,133
Portugal	5,638		6,260	3,325	4,226	2,170
Spain	2,830		9,203	3,920	9,675	7,334
Australia	1,572	9,000	3,406	1,071	905	803
New Zealand	787	—	639	1,140	278	380
Turkey	669	8,000	900	1,468	200	560
Greece	385	400	14,818	5,900	2,100	2,000
Italy	1,652	400	—	—	—	—
Cuba	1,581	4,000	14,409	16,297	13,202	16,002
Novia Scotia	106	200	—	—	—	—
New Brunswick	1,094	1,000	225	—	—	—
Quebec	3	0	—	—	—	—
Great Britain	13,054	10,000	6,000	9,476	6,078	800
Bosnia	4,000	2,000	3,230	1,125	144	—
Holland	1,107	800	—	—	—	—
Other countries	3,114	1,000	—	—	—	—
Japan	—	—	344	935	5,195	15,397
India	—	—	—	—	—	2,508
Totals	147,921	167,680	250,565	182,712	223,773	252,886

TABLE VI.

Manganese Ore Imported into the United Kingdom.

Imported From.	1891. Long Tons.	1892. Tons.	1893. Tons.
Australasia	2,062	1,518	1,183
Russia	48,807	51,854	58,993
Sweden	3,377	3,390	1,133
France	1,434	4,270	8,942
Italy	10	—	—
Portugal	3,105	4,188	2,170
Spain	2,138	11,156	2,804
Turkey	670	1,954	560
U.S.A.	286	—	—
Chili	34,331	27,195	29,365
Japan	—	—	9,194
India	—	—	2,508
Other countries	5,229	4,298	2,200
Totals	101,449	109,823	119,052

[The author is indebted to Mr. J. W. McQueen, of Messrs. McQueen Bros., for information and assistance in the arrangement of the above Tables.]

DISCUSSION.

Mr. S. HERBERT COX, who was unable to be present, sent

museum collections, and find crystallized in cavities in other ores of manganese, but has a massive character, is more of a light brown than a pink colour, and is certainly not what one would have expected to see as a massive deposit of carbonate of manganese. So much was I impressed with this fact when I visited the mines that I feel confident there must be similar deposits as yet undiscovered in other parts of the world where similar conditions prevail. As regards the origin of the deposit, the explanation given by Mr. Hooper is, I should think, the correct one, for of course the carbonates of lime, magnesia, iron, and manganese are isomorphous, and will replace one another in any proportions. It is, moreover, a well authenticated fact that where the limestones of the Ohio petroleum region of America has become dolomitised, in other words, where magnesia has to some extent replaced the lime, the limestone becomes more or less porous, and acts as a reservoir in which the petroleum can be stored, and probably the same porosity would be induced by a substitution of manganese for part of the lime, so that when once this replacement begins it is quite natural to suppose that, given the existence of water carrying manganese in solution, the complete alteration of limestone to diallogite would be the natural outcome. But this does not give us any clue as to the continuation of the deposit, and when Mr. Moreing says that the only course open to him is to follow what he has—he has, in my opinion, said all that he can about it. We are quite unable to predict in what direction limestone caves will extend; they open to large chambers, narrow down to holes through which one can hardly crawl, and again open out to vast halls at various levels, and no geologist and, I venture to think, no mining engineer has ever attempted to predict in what direction the next chamber is likely to be found, and the same remarks apply equally to the deposits of carbonate of manganese in limestone. I have read with a great deal of interest Mr. Moreing's remarks about the method of working that he is adopting, and trust it will prove both successful and economical. It can hardly fail to be an improvement upon the former ridiculous system of working which has been described, and in which the nearest resemblance can be traced in a rabbit warren. There were drives everywhere, and stopes were unknown, while the mineral was handled time after time until it eventually got out of the mine by the Simon adit.

Mr. A. G. Charleton contributed the following remarks, which were read by Mr. G. E. Collins :—

Referring to the geology of the district, I note that the deposit of manganese described occurs in metamorphic rocks of

Devonian age, and my own experience in other parts of the Pyrenees has been that this particular formation carries the principal ore deposits (silver-lead and zinc, as well as antimony and manganese) found up to date in that part of the world. With reference to the proposed method of working, I would venture to suggest that in place of leaving the ground intact, below the drifts, which it is proposed to keep open by means of stone walling, it might be better and cheaper to drive the levels in the first instance in solid ground, and leave a pillar above them, and removing the ore on top of the pillars, in horizontal slices, either by drifting stopes, and filling the spaces up from the foot-wall towards the hanging, or by the method pursued at the Chaplain Mine, Michigan, for example, known as rooming and filling. The ore being sufficiently firm to stand without timbering (in stopes 20 feet wide, extending the full width of the ore body). The rooms are carried up by a series of stopes taken 9 feet high, and after the completion of each stope, the room is filled to within 4 feet of the back; small cars being used to transport the rock filling, to avoid the expense of shovelling. If pillars were left overhead they would of necessity have to be pierced at intervals for passes to communicate with the levels, whilst the waste would, of course, be introduced from above. Supposing the ore to have been all removed above No. 1 level in this manner, and No. 2 level driven, the same operations would be repeated above the pillar forming the roof of No. 2 drive, and the stopes carried upwards till all the ore had been stoped out, up to the original floor of No. 1 drive, which would eventually be replaced by "filling." I should fancy that unnecessary walling and timbering might be saved in this way. I would also like to know if Mr. Moreing can give us figures of the present cost of mining, picking, and

12 miles, and, according to Dr. Foster, are probably all on the same bed, much faulted and contorted. But the more important deposits which I have in mind are situated in the south of Spain, where they were discovered by an English gentleman, a member of this Institution, apparently about the same time as the Pyrenees deposits, and under very similar conditions. The deposits, which are both large and numerous, are in every case the "cores" left in the working out of the black oxide ores into which their upper portions had been altered. As at Las Cabesses, they were universally supposed to consist of barren rock, until examined by the gentleman of whom I have spoken. They seem to be lenticular masses, situated along lines of weakness, and probably consisted originally of carbonate, which has subsequently been altered by siliceous solutions, and the outcrops oxidised by atmospheric agencies. The manganese mines of the Huelva district, as no doubt many here know, have usually more the character of veins, and are specially remarkable for their veinstones of red jasper. This, perhaps, only represents a further stage in the silicification of which I have spoken. As already stated, the deposits are large. In one case, I think, the exposure—at the contact between porphyry and slates—measured 100 by 40 feet. The conditions as regards transport, &c., are distinctly favourable, so that they ought to be able to compete successfully with the Pyrenees deposit, but for one serious drawback. The average Mn. contents are about the same—about 40 per cent. raw. The silica, however, varies from, say, 12 to 20 per cent. and even higher; and this, of course, would be increased by calcining. Hand-picking would, no doubt, reduce it, but it would certainly remain much more siliceous than the Las Cabesses ore. Another drawback is the high percentage of phosphorus—about 0·20. This may, however, be produced by infiltrations from the surface soil, and, if so, would doubtless lessen on getting deeper away from its influence. Judging from the descriptions received the deposits could in most cases be worked by open-cutting. As will be seen from the three little specimens which I have brought, the

ores show the most charming variety in appearance, and also vary considerably in mineralogical composition, the most common being the silicates tephroite and rhodonite, and the carbonate diallogite. I should mention that the gentleman who discovered the deposits thought the latter an alteration product from the former, and that the silicates were of igneous origin. In this connexion it would be interesting to know more exactly the mineralogical composition of the Las Cabesses ore. Mr. Moreing speaks only of diallogite.

Mr. Albert Calvert sent the following remarks, which were read by the Secretary:—

> In reference to Mr. Moreing's interesting paper, manganese is a much more common mineral than, perhaps, the author of this paper would lead us to suppose. It would not be beyond the sphere of possibilities to give at least a thousand localities. It is generally treated by gold, silver, tin, and copper miners as a worthless mineral. Manganese has the greatest affinity for oxygen, and, therefore, it is the natural consequence on the slightest surface decomposition that the carbonates should be replaced by oxides. The author says "an interesting problem for mining engineers is to formulate a reliable theory as to the formation of this deposit, as an accurate knowledge of the mode of its deposition would enable the researches to be continued in an intelligent and scientific manner." With regard to this theory, it was amply handled and exhaustively dealt with by my grandfather in his works between forty and fifty years ago, when he explained the laws governing the mineral solutions, [redacted]

mentioned was the correct ore, though he would like to have had a little more information as to the proportionate amount of iron existing with these deposits. Referring once more to the question of the deposit of carbonate of manganese of the kind mentioned by double decomposition, it would be interesting to know what the full analysis of this mineral was, his impression being that the proportion was pretty much as 5 of manganese to 4 of carbonic acid. He was much struck by the similarity of the two lenticular deposits with the great deposit of cinnabar ore in Almaden. There, there were two lenticular deposits at the surface, which gradually inclined towards one another, ultimately meeting at a considerable depth. Mr. Moreing had also spoken of the various centres of supply of manganese, and had referred to Chili as being the chief one. He thought the Caucasus had been for some time the chief source of supply of rich manganese. Some of the ore was exceedingly rich, but the difficulty in Russia of getting capital, and the still further difficulty of getting trustworthy people to deal with, had prevented many of the deposits from being opened up. The Russian proprietors were not able to deal with the mines themselves, and what with the difficulties of satisfying the tax-gatherers, &c., many of them had been left undeveloped, and some had been abandoned.

Mr. Joseph Garland said the author's subject was one of peculiar interest, and it could hardly be doubted that many members of the Institution knew very little about these remarkable deposits. The paper contained a good deal of information respecting the discovery of the deposits and the old mode of working them, which seemed rather an absurd one. He would have been glad, however, if a few more particulars had been given. The author had not, for instance, told them where the chief markets for the calcined ores were, nor upon what basis the various classes of ores were sold. Mr. Moreing had given a tempting opportunity to formulate a theory of deposition, but had himself hesitated, with his knowledge of the district, to form one, simply

approving of that of Mr. Hooper. The author had stated his opinion that an accurate knowledge of the mode of deposition enabled researches to be continued in an intelligent and scientific manner; but was the converse true? And could not researches be carried out in an intelligent and scientific manner without a knowledge of the mode of deposition? It was not necessary for the intelligent and scientific working of a deposit that the mining engineer should know how the deposit got there. Mr. Moreing himself had stated that the only system of exploitation open was to follow the mineral wherever it went.

Mr. James Mactear said that in the decomposition of carbonate of lime and manganese solutions there would be a very great difference in the bulk of the resulting carbonate of manganese compared to that occupied by the carbonate of lime. Carbonate of manganese would occupy much less bulk. Supposing they had a small spring coming up, the passage through which the spring came would become gradually larger. There would be a constant widening out of the passage of the carbonate of manganese, and the precipitate thus formed was very curious. It settled down apparently into a very hard dense mass, very much resembling London chalk. This mass, which appears to be perfectly solid and perfectly hard—quite hard enough to

these deposits were worked, any explanation which could be offered would be very interesting.

Mr. PERCIVAL FOWLER congratulated the Institution upon the reception of so interesting a paper upon a subject which to all of them must be comparatively new. The author had carefully abstained from giving any details as to the economic workings of the company with which he was connected, and probably there was some reason for this. Mr. Collins had referred to other deposits which could, perhaps, be worked more economically, and they would like to know where these were situated. As far as he could gather, these deposits contained 20 per cent. or 25 per cent. of silica. In the course of his professional experience he had frequently come across what he had thought to be splendid deposits of manganese, but upon analysis they had always proved to be too full of silica for practical use. The ores of Chile and the Caucasus gave an average of about 7 per cent. of silica, which was generally conceded to be about the limit of workable first-class ore. Spain and Portugal had yielded ores of from 8 to 10 per cent., and other countries in varying proportions up to 24 per cent. In dealing with these ores he should wish very much to know whether there was a market for any which contained over 7 or 8 per cent. of silica? Deposits of manganese occurred, in fact, in many countries, but were always rendered commercially valueless by the large proportion of silica. He would like Mr. Moreing to tell them whether the ores with which he had to deal contained just about the limit of silica which is allowed by the large buyers of manganese in this country. He might also tell them what he did with the slag, lime, &c., whether that was a sort of product which he added to the ore and received payment for, and whether it was deleterious or not. He congratulated Mr. Moreing on the method which he had adopted in driving the levels in the way in which he had done. In conclusion he would ask the author whether the walling he had referred to was only on the hanging wall, which he held out as being a dangerous part, or whether it was necessary on all the sides except the footwall.

Mr. G. E. Collins said the amount of silica in the deposits of which he had spoken varied from 12 to 20 per cent.* He understood that the commercial value was not under discussion at present, but only the technical aspect of the question. He had been struck with the similarity in the mode of formation and mineral contents, but had pointed out how the high percentage of silica would handicap these ores in competing with those of Las Cabesses. He thought, however, that as a matter of fact the limit was 15 per cent., with a fine above that to 20 per cent.: this was certainly the case some years ago.

Mr. Moreing expressed his belief that great trouble would be experienced in selling ore containing over 7 per cent. of silica.

Mr. Mactear said there was no doubt that ores of different classes could be sold because they were used for more than one purpose. He had himself used ore which contained a great deal of silica, for the reason that he got it cheaply, and the silica being there in the form of sand helped to make the ore more easily absorbent, and the manganese per unit cost much less. The manganese market now-a-days was practically altogether for steel-making.

Mr. Claude Vautin asked if the value of the fine material would not be increased by submitting them to calcination in revolving furnaces previous to shipment?

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for a description of it to be given, and, accordingly, they must hope to have that at some future time. Some further details as to the source of supply of the coke used and the price of it would have been interesting, as also whether the marble, of which there were samples on the table, had ever been successfully worked; but the points arising out of the discussion were already so numerous that he feared to impose a further burden upon the author.

Mr. Moreing: I am very pleased to find that my paper has called forth so many interesting remarks. I had better proceed to answer *seriatim*, as far as I can, the various questions and points which have been raised. With regard to Mr. Charleton's remarks, he rather finds fault with my method of working the mine, and points out what he considers a better method; but I think he has failed to grasp the fact that the mine was already worked, and that I did not drive the levels to which I have referred, but found them in existence. When I took charge of this mine the 33 metre level was exactly as it is shown on the plan, and I had to fit in the new system of working to utilise the old. Consequently, all Mr. Charleton's very excellent methods for dealing with a new mine are totally inapplicable in this case. Then Mr. Charleton also draws attention to the fact that I have failed to give figures as to the cost of extracting the ore, what it is sold for, who it is sold to, and all the other matters connected with the commercial aspect of the business. I need not say that I have purposely avoided giving any details on this subject, as it is entirely a matter for the directors of the Company. I, as their technical adviser for working the mine, am not going to give to the world their private business as to where their markets are, and as to what they are making. There are many companies and many private owners who would be perfectly willing to allow us to examine their mines and read papers for the general instruction of everybody, as to the formation, &c., but they would not be inclined to let us investigate their books and tell the world what they are making. Mr. Collins seems rather to have confounded, if he will allow me to say so, the silicate of manganese with

the carbonate of manganese. I specially say in my paper that these are the only mines of carbonate of manganese. I know there is plenty of silicate of manganese, but silicate of manganese, as you all know, is an absolutely worthless material—from the point of view of the iron smelter, at any rate—and I must still insist, in spite of Mr. Collins's remarks, that these are the only mines in the world now worked for carbonate of manganese, and in fact the only mines discovered. Mr. Collins states that the Spanish ores contain 20 per cent. of silica, consequently they are worthless for the large trade in manganese for iron purposes. As regards the carbonate of manganese in Wales, the tables which I have given show that there is no manganese of any importance being worked in this country. The whole production of England is only 800 tons, and these specimens on the table show some bands of carbonate of manganese, but the ore is worthless. If it was not worthless, Mr. Collins would probably put it on the market on a large scale.

Mr. Collins: It is worthless just now.

Mr. Moreing: With regard to Mr. Collins's remark about the amount of silica which is permissible from the commercial point of view, I think he is rather behind the age on that subject, because now-a-days, owing to the discoveries of pure ore at these mines, and owing to the pureness of the Chili ores, a high percentage of silica

found in all parts of the world; but Mr. Fowler hit the nail on the head when he said that we all knew of lots of manganese, but when we came to look into it we always found too much silica or too much phosphorus to make it commercially valuable. What I referred to in my paper was carbonate of manganese only—not these oxides, which everybody knows are widely distributed. I now come to Mr. Mactear's remarks, which to me were most interesting, because I, in conjunction with Mr. Hooper, worked out the theory of the formation of the deposit; but I thought it was only right, as he had given a great deal of help to me, to let him have the credit of the theory rather than take it myself. Of course, we are not metallurgists; we are only mining engineers, and are engaged in the practical part of mining. We arrived at these theories from studying other mines and from reading the works of various authorities and applying their theories to what we have seen. I am sorry that I cannot answer some of Mr. Mactear's questions, especially with regard to the amount of iron in the ore, or as to a full analysis of the ore. I was very much interested in what he said about the similarity of the deposits at Almaden and Las Cabesses. He has pointed out the probability of the continuation and richness of these deposits below the present explorations. Of course, it is impossible to say how far this deposit is going down, it may at any moment come to an end; but Mr. Mactear has pointed out that Almaden is very similar, and that it has largely increased in size and richness in depth. We are now down something like 20 or 30 metres below the adit. We are in splendid mineral, and the carbonate ore appears to be richer than the ore higher up, so that I have great hopes now that we shall have the same experience as Almaden; and that, instead of coming to an end, we shall gradually open out into even larger quantities, and that the ore may be of even better quality—at any rate, containing a higher percentage of manganese. Mr. Mactear seemed to think that I was wrong in saying that the main source of supply was Chili, and suggested the Caucasus as being the main source of supply.

Mr. MACTEAR: I spoke of the richness of the supply.

Mr. MOREING: I think there you are wrong, and that you will find that commercially the Chili ore is richer and purer than any ore that comes from the Caucasus. In fact, if you look at the commercial business of manganese to-day, I believe I can say that the Caucasus ore cannot compete with the ores from Chili. The Chili ore fetches a higher price, and is always taken in preference to any ores from the Caucasus, even at a higher price, not because it contains perhaps a higher percentage of manganese, but because it contains less silica and less phosphorus, but the Chili ore is again less pure than the calcined ores from these French mines. These latter are undoubtedly at the present time the purest ores in the market, and there is nothing known at present that can compete with them. Mr. Garland was one of those gentlemen who wanted to know too much. He wanted to know the price, the markets, and all that sort of thing, which I must decline to give him. Then he disagreed with me for saying that the researches would be more likely to continue in a scientific and intelligent manner if we knew how the deposits came there. I think he rather misunderstood what I meant with regard to that. What I intended to imply was that the extensions of the deposit could be more intelligently sought after by knowing how the manganese got there than if we did not know and with all due

various parts of the world. The trouble, as he says, is that they nearly all contain silica and phosphorus and that the only marketable ore now-a-days for blast-furnace purposes is a pure ore. He also wanted to know how we got rid of the lime, and insinuated that possibly we had a way of putting it into the ore and sending it off to market. I may tell him that even if we did this, which we do not, it would do no harm; in small quantities it would not be deleterious, but of course in large quantities it would bring down the percentage of the ore and affect the market value to that extent. He also asked me to explain about the walling. The walling is only to keep the mainway open—not to support the walls of the mine. Mr. Vautin asked about the fine material, and Mr. Mactear also referred to this point. There is no doubt that that fine material could be effectually roasted or calcined by means of revolving furnaces, or by furnaces such as are used for roasting ores at the silver mines in America, but fortunately we find that we can sell the whole of that fine ore in its raw state to blast furnaces in the immediate neighbourhood, and consequently there is no advantage in putting up works to calcine it. The only other remark, I think, was that made by the President, in which he asked about the price of the coke, and where it comes from. The coke which is small and of the very cheapest character, is obtained from the gas works at the nearest town. I do not know exactly what the price paid for it is, but there are several large towns in the neighbourhood, and we find no difficulty in getting any quantity of this small coke at a very low price.

Mr. Moreing concluded by exhibiting some of the various specimens of ore, and explaining their various characteristics.

THE INSTITUTION OF
MINING & METALLURGY.

STATEMENT OF FUNDS,
28th February 1893.

	£	s.	d.
Cash at Bankers, and in hands of Hon. Secretary, as per annexed Statement of Receipts and Payments for the year ended 28th February 1893..	377	19	8
Add.			
Sundry Debtors—not including arrears of Subscriptions and Entrance Fees	20	0	0
	397	19	8
Deduct.			
Creditors	50	0	0
Accumulated Fund, 28th February 1893 ..	£347	19	8

Dr. *Statement of Receipts and Payments*

	£	s.	d.	£	s.	d.	£	s.	d.
To Receipts—viz.:									
Subscriptions—									
Life Members	42	0	0						
Members	243	12	0						
Associates	63	0	0						
Students	29	8	0						
				378	0	0			
Entrance Fees				323	8	0			
Interest on Deposits				1	1	1			
							702	9	1

[redacted]

for the Year ended 28th February 1893. Cr.

	£ s. d.	£ s. d.	£ s. d.
By Payments—viz.:			
Salaries		53 0 0	
Hire of Offices, Rent of Rooms, &c.		13 3 6	
Printing, Stationery, &c.		81 16 11	
Transactions		73 1 7	
Postages and Telegrams		37 0 4	
Travelling Expenses		4 3 11	
Gratuities		2 3 7	
Guests at Inaugural Dinner and Refreshments at Meetings ..		39 10 9	
Expenses of Concert: Repayable by Guarantors		20 0 0	
Bank Charges		0 8 10	
			324 9 5
By Balance, 28th February 1893—viz.:			
Cash			
At Bankers—			
On Current Account	128 3 7		
On Deposit Account	200 0 0		
		328 3 7	
In hands of Hon. Secretary—			
On Petty Cash Account	11 5 9		
On General Account	38 10 4		
		49 16 1	
			377 19 8
			£702 9 1

Examined and found correct,

WOODTHORPE, BEVAN & CO.,
Chartered Accountants, } *Auditors.*

London, *25th July* 1894.

THE INSTITUTION OF
MINING & METALLURGY.

STATEMENT OF FUNDS,
28th February 1894.

	£	s.	d.
Cash at Bankers and in the hands of the late Hon. Secretary and the present Secretary, as per annexed Statement of Receipts and Payments for the year ended 28th February 1894	493	3	11
Add.			
Sundry Debtors, not including arrears of Subscriptions and Entrance Fees	20	0	0
	513	3	11
Deduct.			
Sundry Creditors	160	3	0
Accumulated Fund 28th February 1894 ..	£353	0	11

Dr. *Statement of Receipts and Payments*

	£ s. d.	£ s. d.	£ s. d.
To Balance, as per account, 28th February 1893			377 19 8
To Receipts—viz.:			
Subscriptions—			
Life Members	21 0 0		
Members	185 13 0		
Associates	37 4 0		
Students	23 2 0		
		266 19 0	
Entrance Fees		60 18 0	
Interest on Deposits		3 2 1	
			330 19 1

[redacted]

for the Year ended 28th February 1894. **Cr.**

	£ s. d.	£ s. d.	£ s. d.
By PAYMENTS—viz.:			
Salaries		72 10 0	
Hire of Offices, Rent of Rooms, &c.		15 12 11	
Printing, Stationery, &c.		82 1 6	
Postages and Telegrams		19 19 8	
Travelling Expenses		6 1 8	
Gratuities		1 17 11	
Refreshments at Ordinary Meetings		17 0 0	
Bank Charges		0 11 2	
			215 14 10
By BALANCE, 28th February 1894—viz.:			
Cash—			
At Bankers—			
On Current Account	148 16 4		
On Deposit Account	200 0 0		
		348 16 4	
In hands of the late Hon. Secretary—			
On Petty Cash Account ..	17 8 7		
On General Account	126 6 9		
		143 15 4	
In hands of Secretary—			
On Petty Cash Account ..		0 12 3	
			493 3 11
			£708 18 9

Examined and found correct,

WOODTHORPE, BEVAN & CO.,
CHARTERED ACCOUNTANTS, } *Auditors.*

LONDON, *25th July* 1894.

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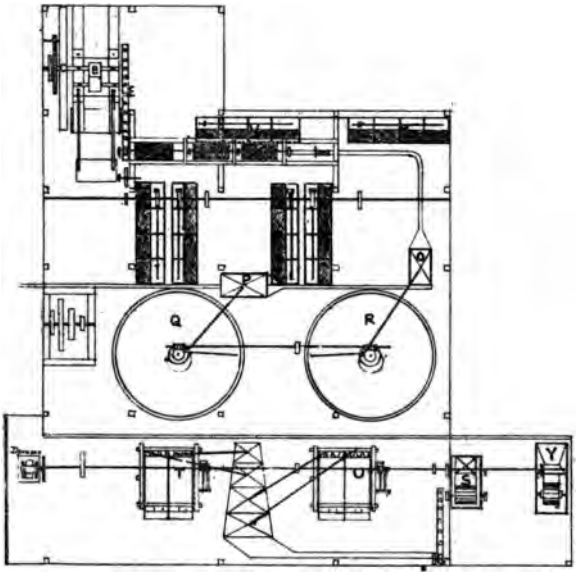
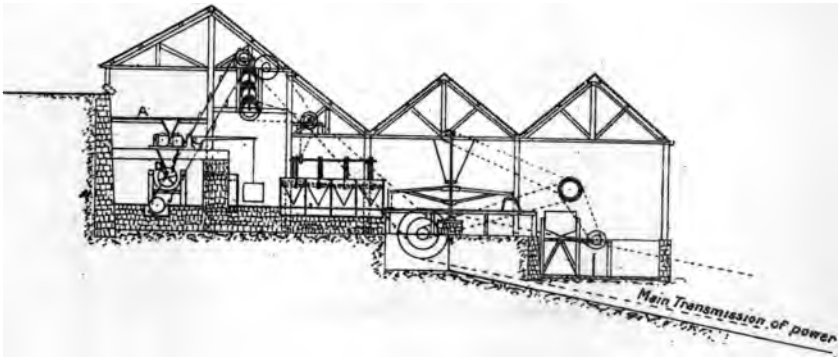


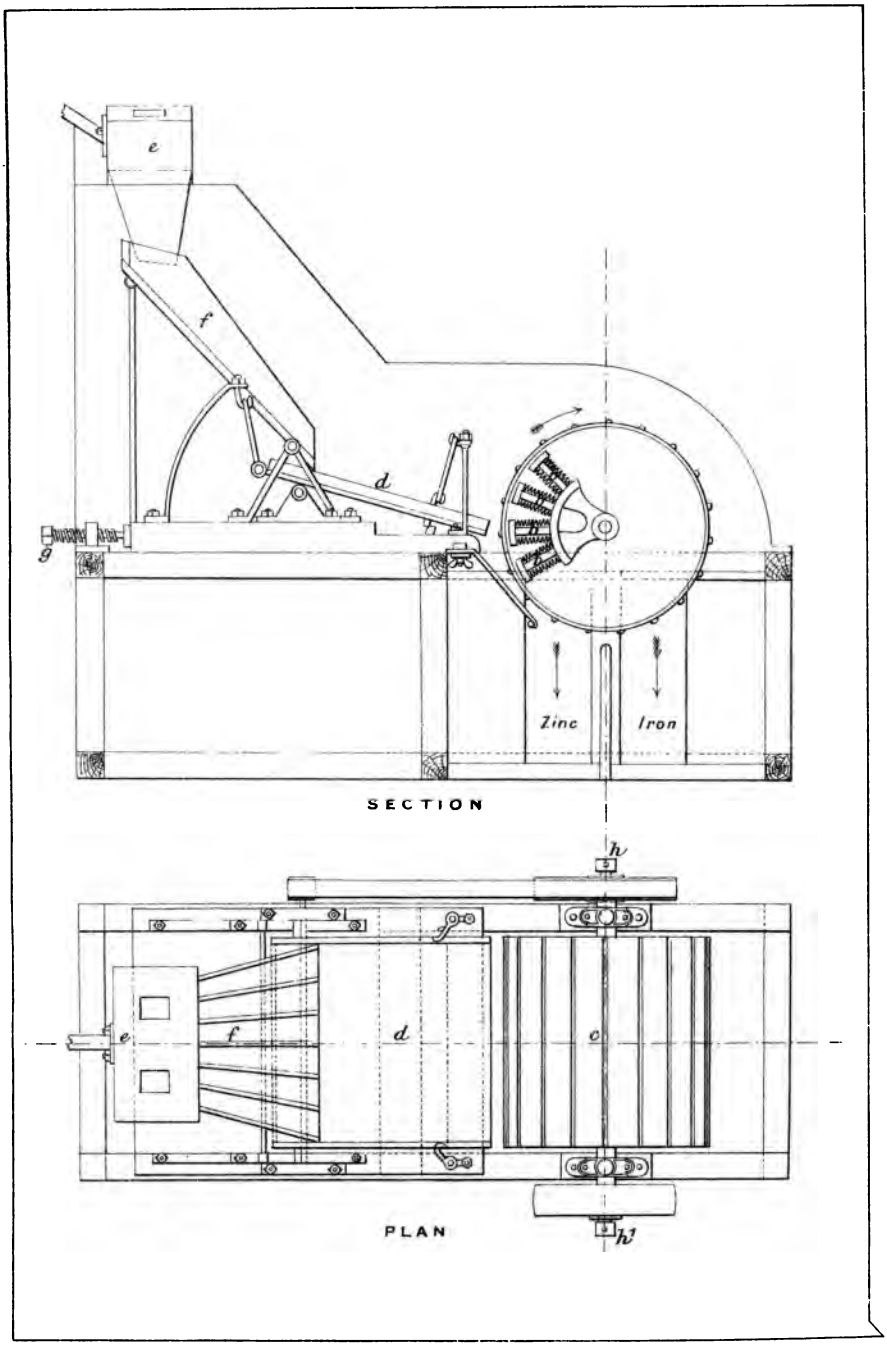
PLAN



SECTION

Illustrating Mr. H. L. Lawrence's Paper on "THE DRESSING OF ZINC-BLENDE ORES."

Plate.II.



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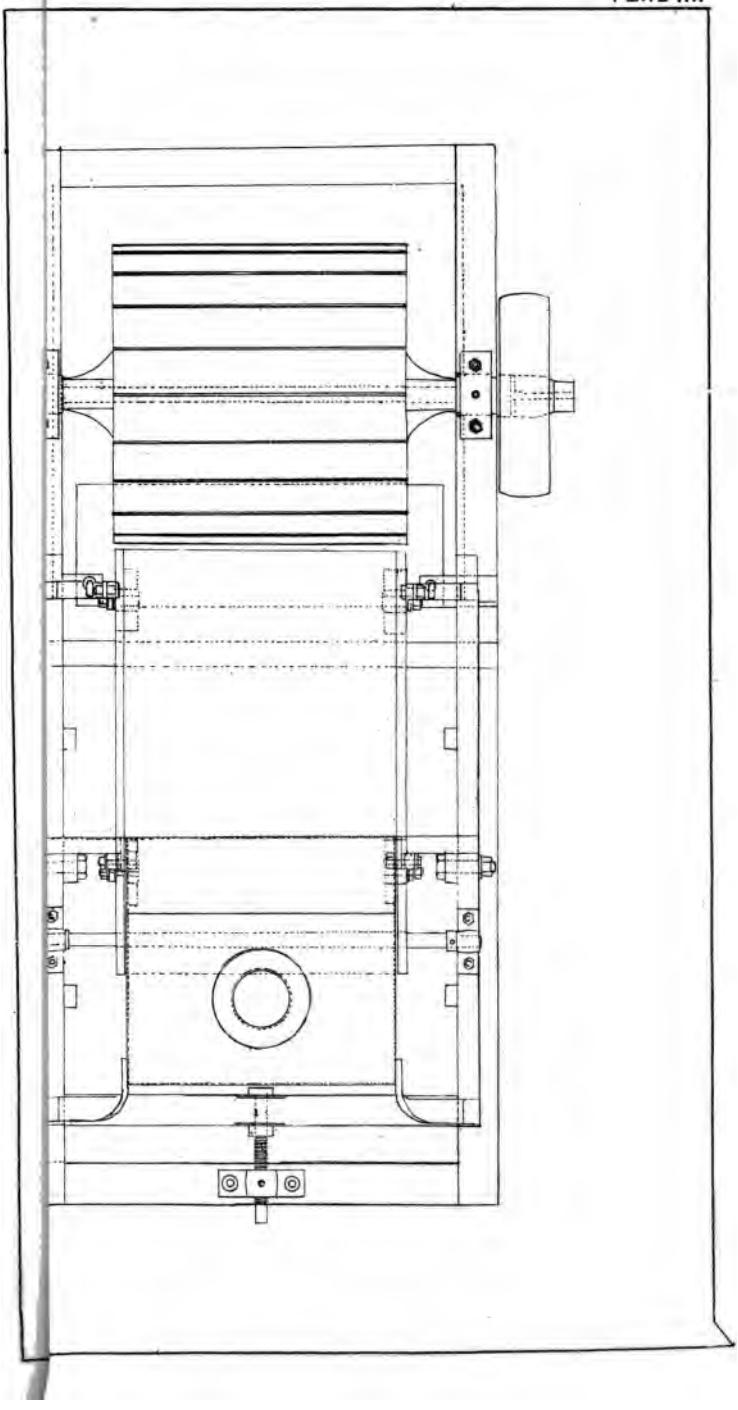

Illustrating Mr. H. L. Lawrence's Paper on "THE DRESSING OF ZINC-BLENDE ORES."

Plate IV

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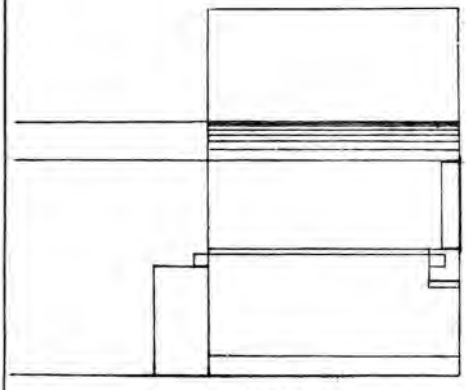

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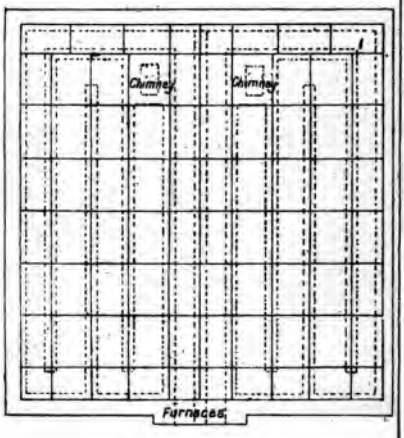
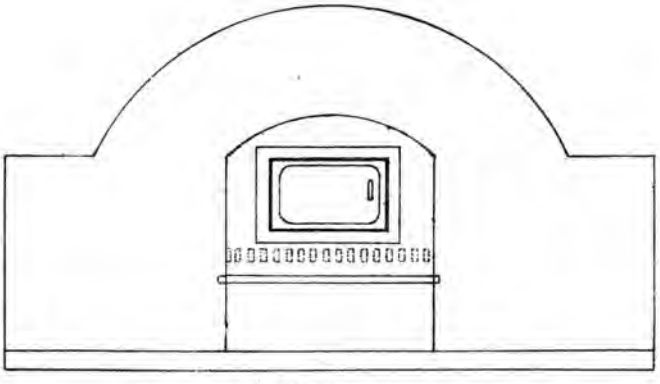
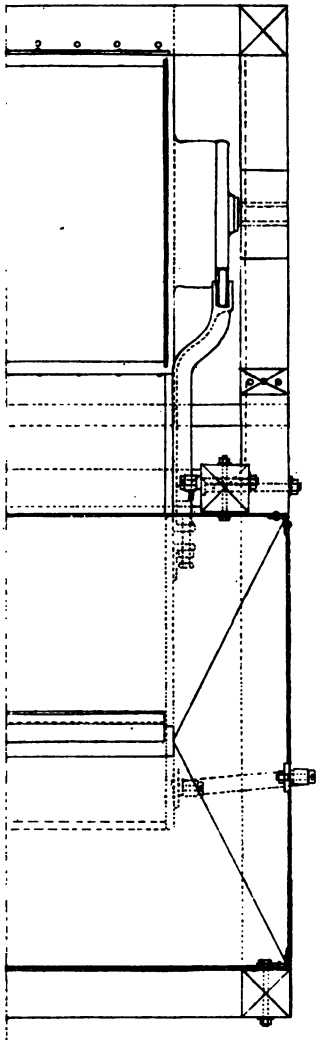


GENERAL PLAN



FURNACE

PLATE V.



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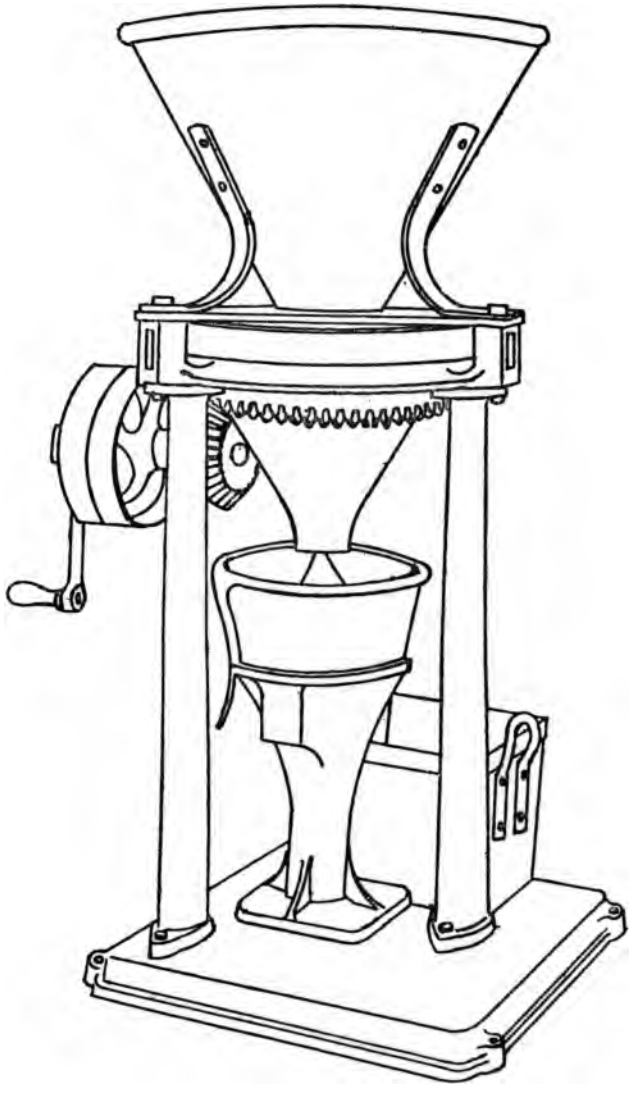

Perspective Sketch of a No. 3 Sampler, with one of the Sample Boxes removed.

Illustrating Mr. C. A. Moreing's Paper on "THE MANGANESE MINES OF LAS CABESSES."

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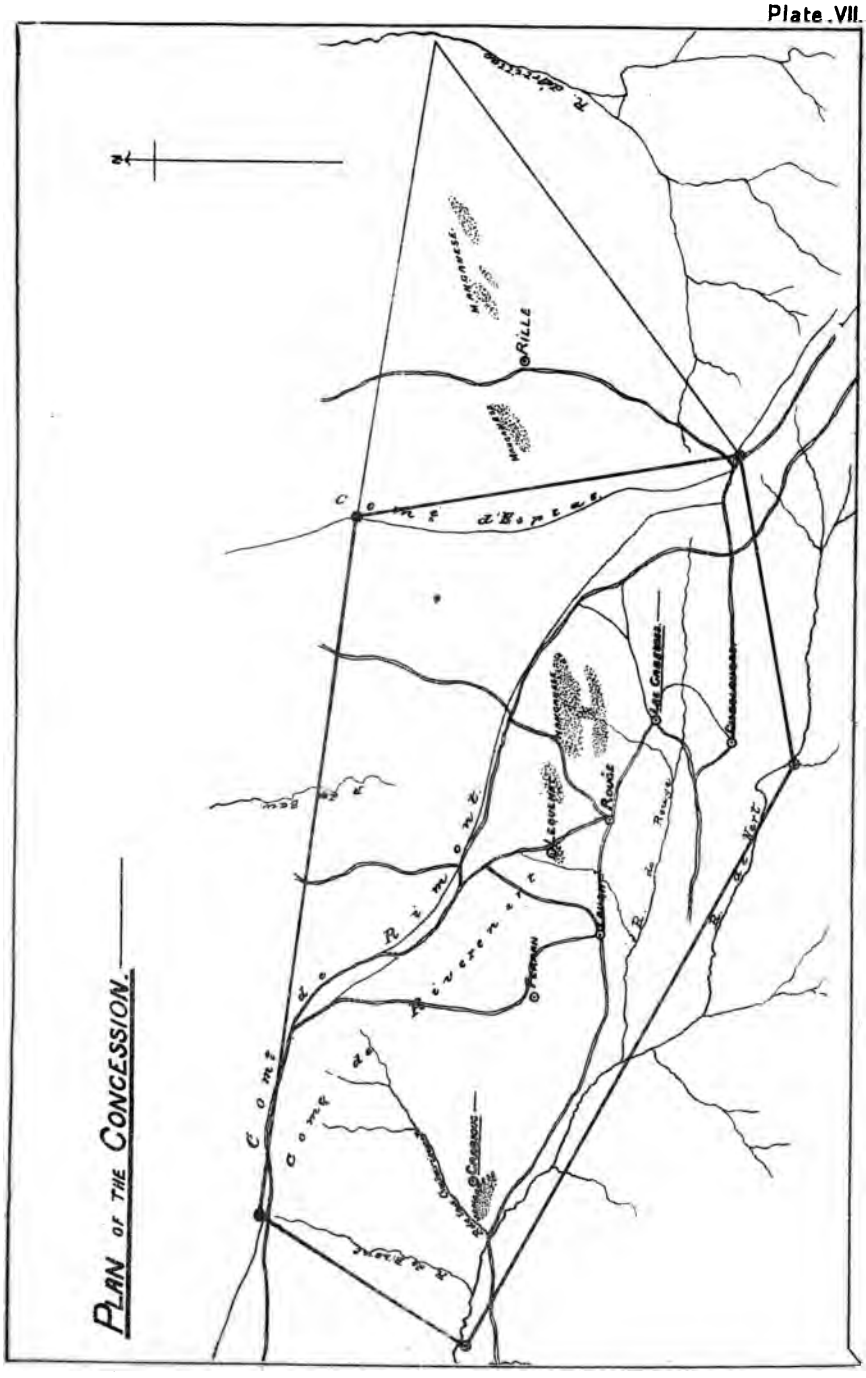

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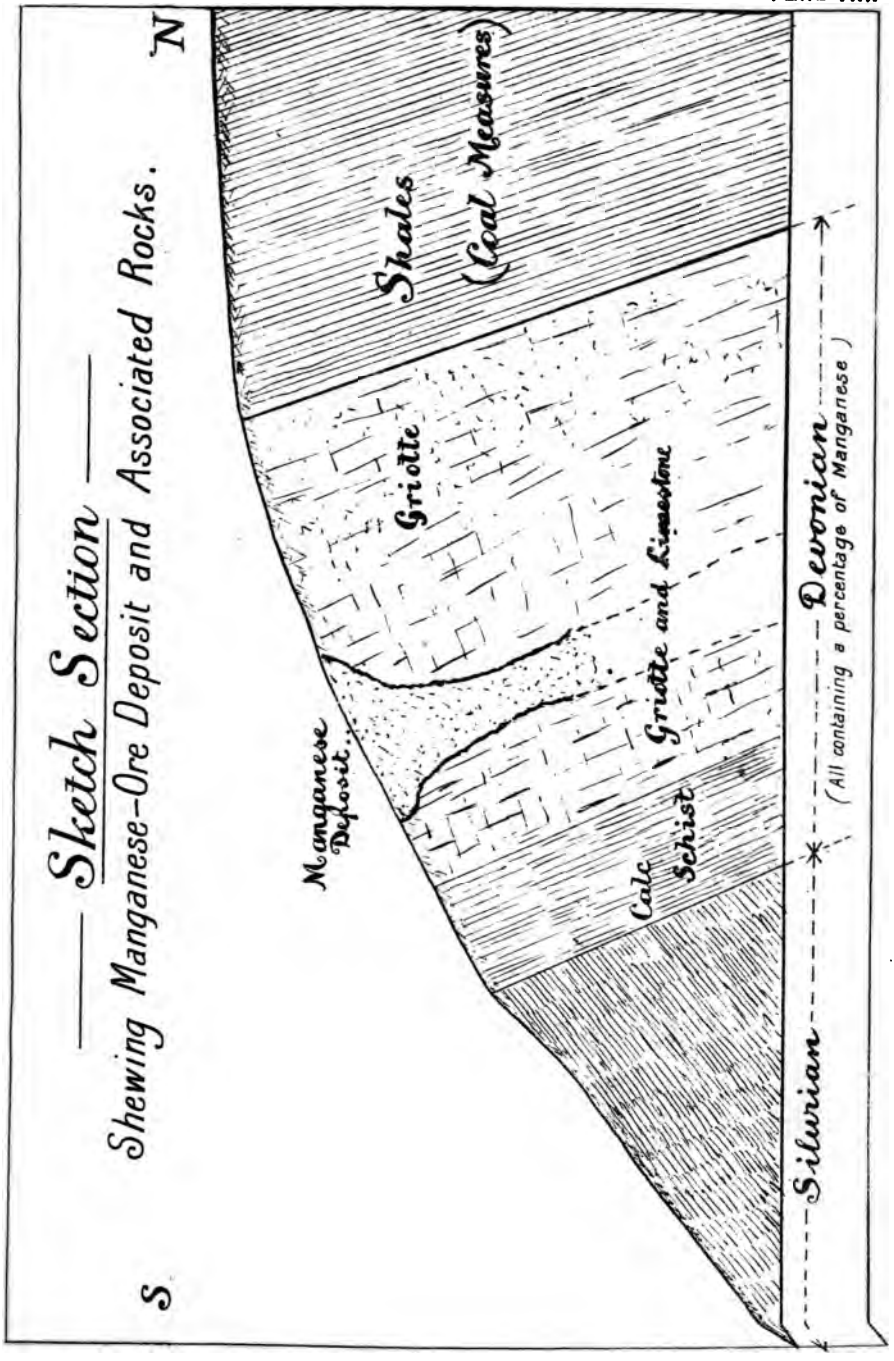

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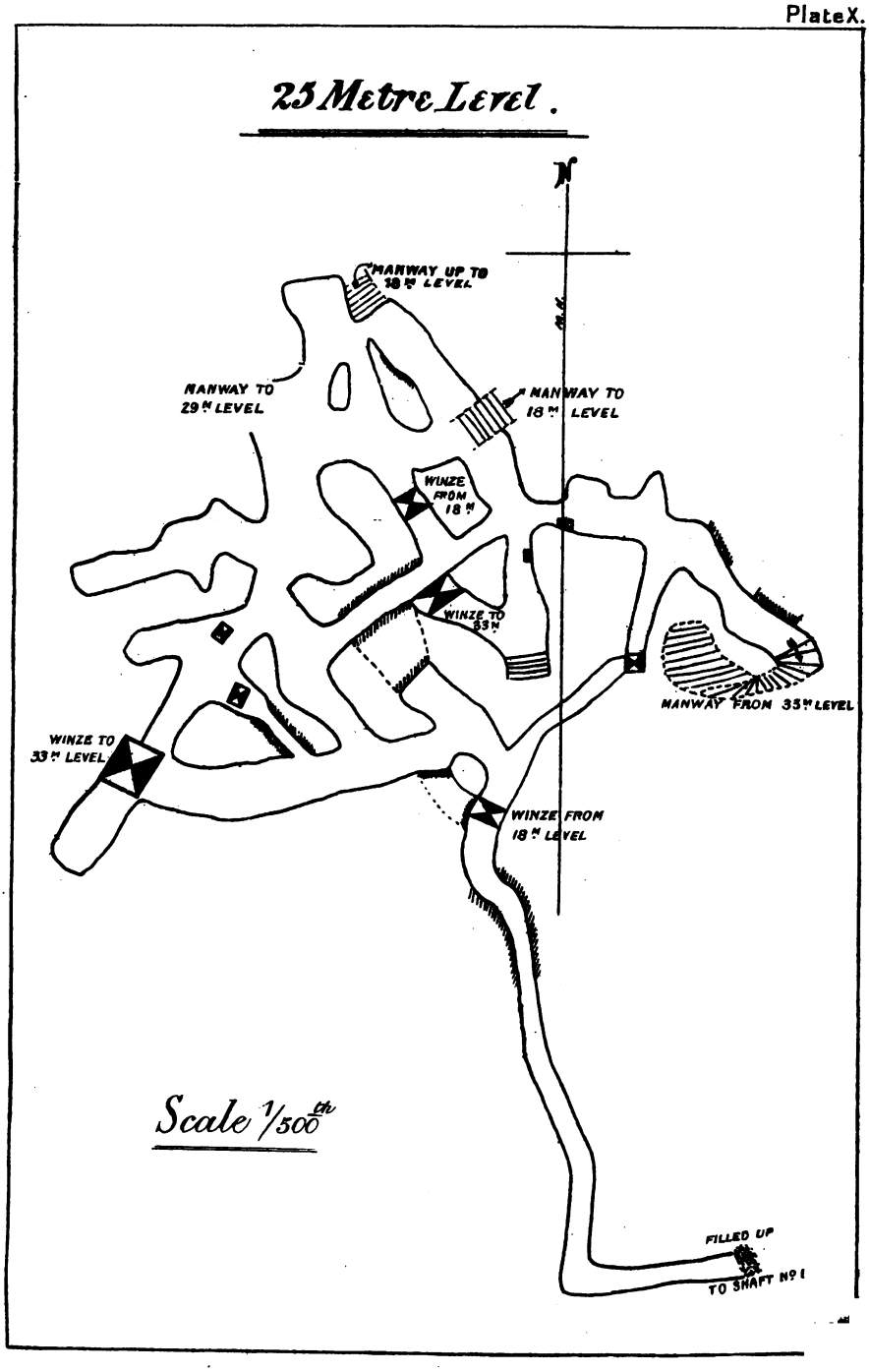

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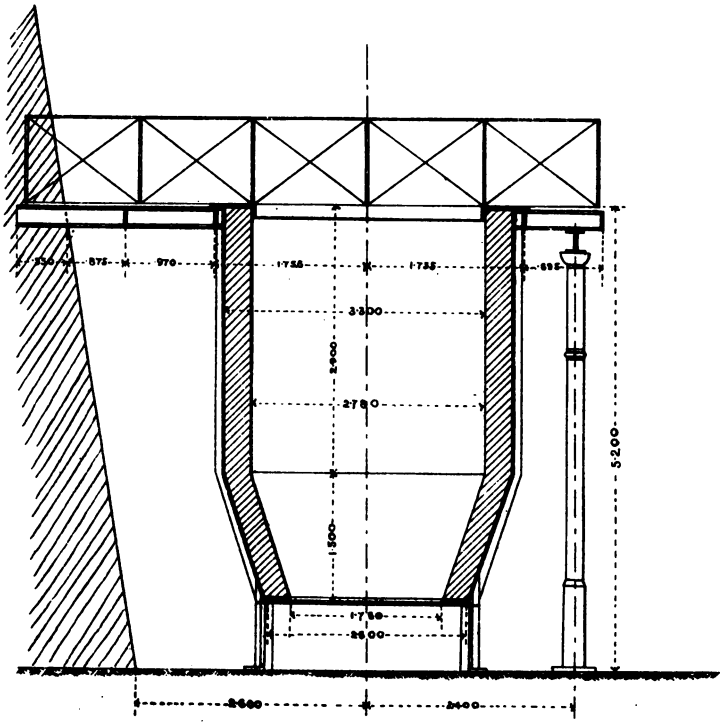
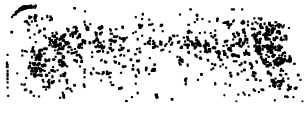
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